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VOLUME TENTH.

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I. *On the Existence of Two New Fluids in the Cavities of Minerals, which are immiscible, and possess remarkable Physical Properties.* By David Brewster, LL.D. F. R. S. Lond. & Sec. R. S. Edin.

(Read March 3. and 17. 1823.)

In the year 1818, my attention was accidentally directed to the subject of water in crystallised bodies, by the explosion of a crystal of Topaz, which I had exposed to a red heat, for the purpose of expelling its colouring matter. This violent disruption of the specimen, which was shivered into a thousand films, of extreme tenuity, arose from the expansion of the imprisoned fluid, and induced me to institute a series of experiments, for the purpose of determining the nature of the fluid, the form of the cavities which contained it, and the arrangement of these cavities in reference to the crystalline form of the mineral *.

Small portions of fluid had been long ago observed by mineralogists in *Topaz*, *Rock-Crystal*, and *Fluor-Spar*. Mr Sivright found them also in *Calcareous Spar*, *Sulphate of Barytes*, and *Sulphate of Lime*; and I detected them in the *Emerald*, in *Beryl*,

* An account of these experiments was announced for publication in 1819, in the 1st Number of the *Edinburgh Philosophical Journal*; but the desire of obtaining more general results prevented me from publishing it at that time.

Cymophane, *Peridot*, *Feldspar*, and in the following crystals formed by aqueous solution.

Sulphate of Iron.	Sulphate of Ammonia and Magnesia.
Sulphate of Zinc.	Nitrate of Silver.
Sulphate of Copper.	Nitrate of Strontian.
Sulphate of Nickel.	Muriate of Barytes.
Sulphate of Soda.	Acetate of Lead.
Sulphate of Magnesia.	Oxymuriate of Potash.
Sulphate of Ammonia.	Muriate of Barytes.
Sulphate of Magnesia and Iron.	Oxalic Acid.
Sulphate of Soda and Magnesia.	Tartrate of Potash and Soda.
Sulphate of Alumine and Ammonia.	Carbonate of Potash.

Being persuaded, from these results, that water will be found in *every* crystal deposited from a solution, I was next desirous of finding it in crystals formed by heat, or by sublimation; but in no case have I been able to discover the slightest trace of its existence; and, in the absence of all other information on the subject, I considered this result as highly favourable to the aqueous origin of those minerals in which water has been discovered.

Sir Humphry Davy was, we believe, the first philosopher who conceived the idea of opening the cavities of crystals, and of examining chemically the nature of the fluid which they contain, and of the gas by which it is sometimes accompanied; and the experiments which he undertook for this purpose, were conducted with that sagacity and address which characterise all his labours. Upon opening the cavities in a variety of rock-crystals of different localities, and collecting the fluids in fine capillary tubes, he discovered, that, in every case, except one, the fluid was *Water* nearly pure;—that, in this single case, it seemed to be *Naphtha*;—that the gas was in two cases *Azote*, and was about 65 times more *rare* than that of the atmosphere;—that, in one case, the gas (the nature of which is not mentioned) was *compres-*

sed about 10 times more than atmospheric air; and that in the naphtha cavity there was almost a perfect *vacuum* *.

Such was the state of the subject, when my attention was again turned to the examination of these cavities.

In resuming this enquiry, I have been fortunate, not only in possessing many excellent specimens of my own, but in having the free use of an interesting collection, belonging to Mr Sivright of Meggetland; and though I have employed only the microscope, and the agency of heat and of light, I have been led to results of considerable generality and interest. This physical method of determining the properties of minute quantities of matter, though often very difficult, and sometimes perplexing in its manipulations, carries with it a degree of evidence not inferior to that of chemical analysis; while it possesses the advantage of examining the substance in its original and unchanged condition, and may be applied, in many cases, where the chemist cannot avail himself of any of the resources of his art.

When the cavities in crystals are very large, which seems to be the case principally when they contain water, the elegant method pursued by the distinguished President of the Royal Society of London will afford precise results, and may be expected to add greatly to our knowledge of this mysterious subject. Leaving, therefore, this branch of the enquiry in the skilful hands of Sir Humphry Davy, I have pursued the subject under a more general form, and have studied the phenomena in their various relations to the principles and methods of general physics.

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* *Philosophical Transactions*, 1822, p. 367.

Sect. I. *On the Existence of a New Fluid in the Cavities of Minerals.*

In examining the cavities of crystallised bodies, I observed the most striking difference in the phenomena presented by the fluids which they enclosed. Impressed with the opinion that the fluid was water, I tried every method of explaining, upon ordinary principles, the phenomena which were thus presented to me, but the results of a more minute examination were incompatible with such a supposition, and rendered it necessary to ascribe them to new fluids, possessing new physical properties. In order to convey to the Society a correct idea of the methods of observation, and the train of reasoning by which I was led to this conclusion, I shall give a detailed account of the phenomena, as exhibited in different minerals.

1. *Topazes from New Holland, Scotland, and Brazil.*

As the cavities in the New Holland topazes are frequently arranged in strata parallel or slightly inclined to its most eminent cleavage, or the one perpendicular to the axis of the prism, they are peculiarly fitted for carrying on this enquiry. The facility with which this mineral may be split, allows us to dispense with the aid of the lapidary, and to study the phenomena through perfectly flat and highly polished surfaces.

In examining these specimens with the microscope, we observe the cavities arranged in strata. These cavities are sometimes beautifully crystallised, and sometimes amorphous, sometimes extremely shallow, and at other times deep. They have often the shape of long canals, with parallel sides and round

terminations, and at other times their form is not far from that of a circle.

The cavities now described, are filled with a colourless and transparent fluid, as shewn at ABCD, Fig, 1. Plate I., and have almost always a vacuity V, of a circular form, which moves by an inclination of the plate to different parts of the cavity. The depth of the cavity may be easily estimated, by the breadth of its bounding line ABCD, which, in the flat cavities, is generally the same as that of the circle V. In very shallow cavities, this boundary is a narrow line, scarcely visible, and in deep ones it is broad, with a penumbral termination inwards, arising from the deviation of the light at the separating surfaces of the fluid and the topaz, and at that of the fluid and the vacuity.

When the hand is applied to the crystal, the heat of it gradually expands the fluid. The vacuity V consequently diminishes, and being in a short time reduced to a physical point, it entirely disappears. When the fluid again cools, by withdrawing the hand, it of course contracts, and quits the sides of the cavity. The vacuity V reappears, increasing till it resumes its former magnitude; and it deserves particular notice, that the evanescence and reappearance of the vacuity takes place simultaneously in many hundred cavities, of the same general form, which may be seen in the field of view.

In order to obtain an accurate measure of the temperature at which the vacuity reappears, which is almost the same as that at which it vanishes, I plunged the topaz in heated water, and by means of an accurate thermometer, I obtained the following results:

Nature of the Cavities.	*Temperature at which the Vacuity reappeared.*
1. Topaz from New Holland, with shallow cavities,	74 ½° Fahr.
2. Blue Topaz from Aberdeenshire, with cavities of different forms, - - -	74 77 78 ½ 82

Nature of the Cavities.	*Temperature at which the Vacuity reappeared.*
3. Colourless Topaz from Brazil, with only one cavity $\frac{1}{20}$th of an inch long, $\frac{1}{62}$d of an inch broad, and $\frac{1}{38}$th of an inch wide, -	79 $\frac{1}{2}$
4. Topaz from New Holland, with large and rugged cavities, - - -	79 $\frac{3}{4}$
5. Topaz from New Holland, with a very flat cavity, - - -	81 $\frac{1}{4}$
A very long and very irregular cavity in the same crystal, - - -	82 $\frac{1}{4}$
6. Another colourless Topaz from Brazil, -	83 $\frac{1}{3}$
7. Another colourless Topaz from Brazil with a deep cavity,	83 $\frac{3}{4}$

The reappearance of the vacuity at different temperatures in different cavities of the same crystal, admits of an easy explanation. In those which are of the same size and form, and equidistant from the cooling surface, the vacuities disappear at the same time; but in those which are deep, and in those which, though shallow, are near the cooling surface, the vacuities reappear at a lower temperature. In very shallow cavities, the adhesion of the fluid to the sides of the cavity prevents the vacuity from reappearing so soon as it would otherwise do; while in cavities that have a rough or irregular bottom, they reappear earlier.

When the cavities are very small and narrow, only one vacuity reappears; but when they are large, several small circular vacuities make their appearance, and gradually unite into one, though sometimes they remain permanently separate. When the cavities are deep, a very remarkable phenomenon accompanies the reappearance of the vacuity. At the instant that the fluid has acquired the temperature at which it quits the sides of the cavity, a rapid ebullition takes place, and the transparent cavity is for a moment opaque, with an infinite number of minute

vacuities, which instantly unite into one vacuity, that gradually goes on enlarging as the temperature diminishes.

In order to determine the expansion which takes place by a given increment of temperature, I measured the relative size of the vacuity, and the cavity at the temperature of 50° and 80°, the temperature at which the fluid had expanded so as wholly to fill the cavity. In many cases this could be estimated with tolerable accuracy, and it may be stated in general, from the estimates and measures taken by myself, and by others, to whom I shewed the cavities, that the fluid expands fully *one-fourth* of its size, by an increment of 30° of heat.

Hence, since water expands $\frac{1}{22}$d of its bulk in passing from 41°, its state of maximum density, to 212°, it will expand $\frac{1}{125}$th of its bulk for 30°, and $\frac{1}{125} \div \frac{1}{4} = 31\frac{1}{4}$; that is, the fluid contained in the cavities is above 30 times more expansible than water, by an increment of 30° of heat at the temperature of 50°.

This extraordinary result proved beyond a doubt, that the substance contained in the cavity was a new fluid, differing from all known fluids in its high expansibility, and resembling in this respect a gaseous more than a fluid body.

In order to confirm this result, I was desirous of examining the other physical properties of this remarkable substance. I could not fail to notice, in the deep cavities especially, the singular volubility of the fluid, and its slight adherence to the sides of the cavity, as indicated by the motion of the vacuity V. In small cavities containing water, the adhesion of the fluid to the stone is so strong, that the air-bubble moves with extreme difficulty, and even when very large, it often changes its place by starts, or remains stationary at the bottom or in the middle of the cavity. In the present case, however, the vacuity moved about with great facility, and in the cavity, $\frac{1}{20}$th of an inch long, by $\frac{1}{78}$th and $\frac{1}{62}$d of an inch wide and deep, the slightest tap of the

finger on the microscope caused the air-bubble to tremble and oscillate in this miscroscopic level. Hence the new fluid is distinguished by a second physical property, no less remarkable than the first.

Although I now entertained no doubt of the accuracy of the conclusion, that the fluid was a new one, yet I conceived it might be possible to obtain at least an approximate measure of its refractive power, and thus to put its novelty beyond the reach of a doubt. In order to do this, it became necessary to observe the manner in which the total reflexion of the upper surface of the cavity was modified by the contact of the fluid, and, if possible, to measure the angle at which total reflexion was effected, by the separating surface of the fluid and the solid. For this purpose I took a plate of topaz AB, Fig. 2., with a stratum of cavities *m n*, perfectly parallel to the natural surface of the plate. I then placed upon each surface the rectangular prisms ABC, ABD, and introduced between them a thin film of oil of cassia. Rays of light RS, RS were then allowed to fall upon the stratum of cavities *m n*, so that the rays reflected from the upper surface of the cavity could be examined by a microscope whose object lens is LL. Upon making this arrangement, the stratum of cavities was seen in the most beautiful manner. The vacuity V, Fig. 3. of a cavity seen in this way, shone with all the brilliancy of total reflexion, the separating surface of the new fluid ABCD, and the cavity, exhibited a faint grey tint, while the surrounding portions of the solid topaz were comparatively black. The variations which the vacuity V undergoes by heat are now finely seen, and at a temperature of 80° it vanishes in a brilliant speck, leaving the whole of the cavity ABCD of the same uniform tint as in Fig. 4.

The phenomena now described are not so distinctly seen when the stratum *m n* is deeply seated beneath the surface of

the topaz, in consequence of the duplication and overlapping of the images formed by double refraction.

This inconvenience, however, may be nearly removed, by making the plate of topaz very thin; or it may be entirely remedied, in plates of any size, by causing the incident rays RS RS, Fig. 2., to pass along one of the resultant axes of the topaz, while the reflected rays SL SL pass along the other resultant axis.

In order to compare the angle at which total reflexion took place at the upper surfaces of the fluid and the cavity, with that which would have taken place had the fluid been water, I placed a drop of water on part of the lower surface of the plate AB, and I found that the light reflected at the same angle of incidence, was much more brilliant from the separating surface of the new fluid and the cavity, than from the separating surface of the topaz and the water, a result which indicated, in the most unequivocal manner, that the new fluid had a refractive power inferior to water, and that it differed in this respect from every other known fluid.

Although, in this estimate, I attended carefully to the circumstance, that, in the one case, the light reflected from the bottom of the cavity was combined with that reflected from its surface, and therefore used deep cavities, where the two reflexions could to a certain degree be separated; yet, in order to remove any doubt that might remain on the subject, I took a plate of topaz that contained water, or, to speak more correctly, a fluid which did not expand by heat, and upon comparing the reflexions from the cavities, the difference was most palpable.

In one specimen I measured the difference between the angles of incidence at which total reflexion took place, at the separating surface of the new fluid and topaz, and at the separating surface of water and topaz, and I estimated that the refractive power of the new fluid was below 1.300, that of water being 1.336.

In one specimen of *Amethyst* I was enabled to determine, that the angle of total reflexion took place at 51° 26′, and, consequently, that the refractive power of the Fluid, or m, was $= m' + \sin 51° 26' = 1.21066$, m' being taken equal to 1.5484, the ordinary refractive power of Amethyst.

Many other details might have been added under the present head, in support of these conclusions; but they are necessarily reserved for the next section, with which they have a more immediate connection.

2. *Cymophane or Chrysoberyl from Brazil.*

In several specimens of this mineral I have discovered strata of cavities, which contain the new fluid. One of these is remarkable for having two strata parallel to one another; one of which, about $\frac{1}{7}$th of an inch square, contains no fewer than 30,000 cavities, filled with the new fluid, which expands and fills the cavity with the heat of the hand. The cavities are in general very small; but I succeeded in determining that the vacuities all reappear simultaneously, at a temperature of $83\frac{1}{4}$°.

3. *Quartz-Crystals from Quebec.*

In examining the crystals of quartz from Quebec, I have found that almost every specimen of it contains cavities with the new fluid.

In one crystal the vacuity reappeared at 76°. Another vacuity in the same crystal reappeared at 80°; while another, almost in contact with this, required a temperature of 125° to make the fluid fill the cavity.

In another specimen of the crystals there are cavities where the new fluid expands fully $\frac{1}{3}$d of its bulk, by an additional temperature of 30°; and though they are very shallow, the vacuities reappear in the form of several smaller vacuities, and exhibit an appearance as if the fluid were thick and viscid.

While I was applying a heat not above 170° to some of these specimens, they frequently leapt from the plate of glass on which they lay, and at other times threw off, with an explosion, considerable fragments. In one of these experiments, I was fortunate enough to observe a phenomenon which will be considered a very remarkable one. When the compound microscope was adjusted to a distinct view of a stratum of globules containing the new fluid, but particularly to a vast number of minute specks, which the microscope had not power to resolve, a heat of about 150°, which happened to be applied to the specimen, produced a sort of crackling noise, which arose from the bursting of the cavities near the surface. Upon looking into the microscope, I was astonished to observe a great number of *darkish brown* globules rising through the solid quartz, like globules of air in water. In examining them more minutely, I observed, as shewn in Fig. 5., that they took their origin from the minute specks or cavities, which gradually enlarged and went off in the form of a globule. This phenomenon lasted fully five minutes, when the specimen burst into two or three pieces. While examining the cavities of one of the fragments, I found that several of the large ones, with flat faces, had been emptied of their contents, through a fissure parallel to their flat faces, and that the faces of the fissure had closed up, so as to transmit a brownish light, while a bright light was freely transmitted through the polished faces of the cavities as shewn in Fig. 6 *.

Had only one of the cavities shewn in Fig. 6. existed, the fluids which it contained might have escaped through a narrow fissure, not wider than its own breadth; and this narrow fissure

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* In a specimen of Topaz which split in the fire, I found that a quantity of the new fluid had got into a fissure, where it has been permanently detained without reaching the surface. It exhibits the same brown tint as the globules, at particular inclinations.

might, in virtue of the elasticity of the stone, have closed up completely, so as to transmit the light as freely as if it had never existed. This process is by no means a hypothetical one. I have repeatedly formed these fissures in glass, and have sometimes seen them close up in a few minutes, without leaving a trace of their existence behind. When they are wide, a day, a week, and sometimes a month was necessary, to effect the reunion of their sides *.

These circumstances enable us to give a satisfactory explanation of the remarkable phenomenon represented in Fig. 5. When the expansive force of the imprisoned fluid was sufficient to make it penetrate the stone, it would probably escape at the weakest point of the cavity, and pass to the surface through a narrow channel, which the elasticity of the stone would immediately close up. These fissures would probably lie in the direction of the cleavage, and this seemed to be the path which the globules took in their oblique ascent, as represented in Fig. 5.

4. *Amethyst from Siberia.*

The greatest quantity of the new fluid which I have yet seen, exists in a specimen of *Amethyst* belonging to THOMAS ALLAN, Esq. This very interesting specimen is represented in Fig. 7., where *a*, *b*, *c*, *d*, *e*, represent five cavities parallel to each other. The largest of these is $\frac{10}{55}$ of an inch in length, and $\frac{1}{27}$th of an inch in breadth; and the vacuity is about one-fourth part of the whole cavity. By the heat of the hand the fluid swells, and fills all the cavities, and when the vacuity has been considerably reduced by heat, it moves from one end of the cavity to the other, with a degree of volubility truly surprising. By a

* A full account of these experiments will be found in the *Philosophical Transactions* for 1816, p. 73.

careful experiment, I found that the vacuity disappeared at a temperature of $83\frac{1}{3}°$; and when it was made to reappear by rapid cooling, an ebullition took place, as in the deep cavities of topaz.

As the cavities in this specimen were terminated with crystallised summits at *a*, *b*, *c*, *d*, I was enabled to observe a curious optical phenomenon, which accompanied the expansion of the fluid. Whenever the vacuity was so much reduced by the expansion of the fluid, that it could be made to occupy one of the crystallised summits, and afterwards to vanish, it left behind it, on that summit, a system of beautiful concentric coloured rings, which were constantly varying in tint, in diameter, and in number. These rings had the highest order of colours in their centre, and continued while the fluid preserved its expanded state; but they invariably disappeared when the fluid was allowed to contract by cold, as if the substance which formed them had assumed a gaseous form, and entered into the vacuity.

Sect. II. *On the coexistence of two Immiscible Fluids, of different Physical Properties, in the Cavities of Minerals, and accompanied with a vacuity.*

Although many of the cavities which have been described in the preceding section, contain only the new fluid, yet in a very great number, particularly in Topaz, another phenomenon presents itself, which requires a very minute examination. This phenomenon, as exhibited in Topaz, is represented in Figures 8, 9, and 10, where V is the vacuity, NNN the new fluid, and WWW another fluid, which we shall distinguish by the name of the *Second Fluid.* This second fluid WW commonly occupies the angles of triangular cavities, as in Fig. 8., or the terminations of longitudinal ones. It is always separated from the new fluid by a curved surface *m n*, *m n*, &c. It never expands perceptibly

with heat, and never mixes with the new fluid NN. By a little management, the vacuity V may be made to come in contact with the bounding lines *m n*, *m n*, &c.; but it never affects its curvature, and seldom enters the fluid W. When the vacuity V has been made to vanish by heat, these bounding lines remain exactly the same.

Having at first observed this second fluid only in the angles of cavities, as in Fig. 8., I experienced considerable difficulty in establishing its fluidity. The improbability of two fluids existing in a transparent state, in absolute contact, without mixing in the slightest degree, induced some of my scientific friends to refer it to an optical illusion, and to consider the line which separated it from the new fluid as a septum or partition in the cavity. The beautiful curvature of the bounding line, and its perfect similarity to that of two contiguous fluids, rendered this conjecture untenable. It was next supposed to be a vacuity into which the new fluid could not expand itself; but though this idea explained the curvature of the bounding line, it was inconsistent with other facts, and especially with the important one, that the second fluid acted upon light neither like topaz nor a vacuum, but like water.

These difficulties were gradually overcome by more numerous observations.

Although the cavities were generally like those in Fig. 8., where V is the vacuity, NN the new fluid, and WW the supposed second fluid; yet I found several in which the second fluid filled a great part of the cavity, as in Fig. 9., where NN is the new fluid, and W the second fluid, or as in Fig. 10., where a vacuity V also appeared within the globule N of the new fluid.

This great enlargement in the quantity of the second fluid, removed most of the difficulties which had formerly presented themselves; but something was still wanting to prove its fluidity. This desideratum was fortunately obtained in a specimen of to-

paz belonging to Mr Sivright. In examining this specimen, I observed a very remarkable cavity, of the form shewn in Fig. 11., where A, B and C are three separate portions of the new fluid, insulated by the interposition of the second fluid DEF. The first portion A of the new fluid had four vacuities V, X, Y, Z, while the other two portions B, C, had no vacuity. Having often succeeded in making the vacuities pass from one branch of a cavity to another branch, I did not doubt that the vacuities of the portions B and C had passed over the second fluid into the portion A. In order to determine this, I took an accurate drawing of all the phenomena at a temperature of 50°, as represented in Fig. 11., and I carefully watched the changes which took place, by raising the temperature to 83°. The new fluid at A gradually expanded itself, till it filled up all the four cavities V, X, Y, Z; but as the portions B, C, had no cavities for this purpose, they could only expand themselves, by pushing back the supposed second fluid DEF. This actually happened. The second fluid quitted entirely the edge of the cavity at F. The two portions of new fluid B, C, were immediately united into one; and the second fluid having retreated to its new limit *m n n′ o*, and being itself but slightly expansible, like common fluids, its other limit necessarily advanced to *p q r*. This experiment, which I have often repeated, and shewn to others, involves one of those rare combinations of circumstances, which Nature sometimes presents to us, in order to lay open some of the most mysterious of her operations. Had the portions B, C, of the *new fluid* been accompanied, as is usual, with their vacuities, the interposed *second fluid* would have remained immoveable between the two equal and opposite expansions: but from the accidental circumstance of these vacuities having passed over into the other branch A of the cavity, the *second fluid* is placed in a sort of unstable equilibrium, and, like the arms of a lever, it yields to every variation of the power and of the resistance.

If any additional evidence were wanted on this subject, we have only to examine the mode in which the two portions of the new fluid B, C, are united into one by a disunion of the second fluid at *g h*, and again separated by its reunion. Upon the application of heat, the summits *g*, *h*, become more acute, and gradually approach to each other, till they suddenly unite, and force back the surface of the second fluid into the line *m n n′ o*. A portion of the second fluid, however, is retained by capillary attraction, in the angular meeting of the planes, between *c* and F, and between *d* and F, and also a small portion at *f*, a phenomenon which affords an ocular explanation of the immobility of the second fluid in the terminations and angles of cavities. When the fluids again cool, the surface *n n′* approaches to *c d*, and when *n* is near *c*, the surface *n n′* of the second fluid and that of the same fluid in *c d*, suddenly start into union, in virtue of their mutual attraction, and the portions B and C are again separated.

By allowing the specimen to rest in particular positions, I have often driven part of the vacuity V towards X, so as to unite all the three vacuities X, Y and Z into one; and in like manner I have caused the vacuities Y, Z and part of X to disappear and unite with the vacuity V.

In order to examine the refractive power of the second fluid, I made the arrangement represented in Fig. 2., and found that the second fluid W always reflected less light than the new fluid, and consequently that its refractive power approached nearer to topaz than the new fluid. By the same means I determined, that the angle at which total reflection took place at the separating surface from the topaz, was very nearly the same as if the second fluid were water.

The fortunate circumstance of the cavities B, C, being without a vacuity, and the consequent mobility of their bounding lines *a b c*, *d e f*, enabled me to compare the optical properties of the

two fluids, by means of transmitted light. The sides of the cavity being inclined to one another, like those of a prism, it is manifest, that if *a b c* is the boundary of two fluids of equal refractive power, the image of a luminous object will have the same deviation, by the refraction of both. As the cavity, however, is too minute to permit an image to be distinctly seen through it, it becomes necessary to look with a microscope at the illumination of the surface of the cavity, and if the two refractive powers are *equal*, the portion above *a b c* will be *dark*, when the portion below it is *dark*, and *vice versa*. I found, however, that the portion of fluid B *a b c* was often *dark*, when the second fluid below *a b c* was *light*, and I therefore concluded that this arose from their unequal refraction. To this conclusion it may be objected, that the inclination of the refracting faces might accidentally be different behind B *a b c*, although it is not likely that the portion possessing this difference of inclination would be bounded by a curve line *a b c*. I therefore applied heat to the specimen, and, by expanding the new fluid at B and C, the bounding lines were made to move from *abc*, *def*, to *mnn′o*, and I remarked, that, during this change of position, the boundary of the two fluids was always the boundary of the unequal shades produced by unequal refraction.

As the arrangement of the fluids which enabled me to make these experiments, possesses a peculiar interest, I have carefully looked for similar cavities, but I have not succeeded in finding more than a few examples, one of which is represented in Fig. 12., as it appears at the temperature of 32°. This cavity consists of two wide portions, separated by a narrow channel. The new fluid occupies the portion between *c c*, *d d*, and also that between *a a* and *b b*, these two portions being separated by the second fluid *d d*, *a a*. The whole vacuity exists at V. If we now apply heat, the new fluid at N and N expands, and the boundaries *d d*, *a a* and *b b*, advance towards B. The vacuity V becomes

an elliptical bubble, and finally vanishes. When this takes place, the boundary *b b* has of course disappeared, and *d d* and *a a* have advanced to *d′d′* and *a′a′*, and *c c* is invisible, in consequence of the new fluid having spread over it, as it were, in the manner described in the following section.

Another cavity, consisting of three separate portions, AB, CDE, FGHK, is shewn in Fig. 13., and is remarkable, in consequence of each of these masses being connected with the adjacent one, by a portion of the second fluid, which moves between them like a piston through the extremely narrow channels BC, EF. As the portion of new fluid between *a b* and *e f* expands without having an air-bubble, it pushes the portion of the second fluid B *a b* through BC into C *a′ b′*. In like manner, the second fluid *c d* EF *c′ d* varies its position with the expansion of the fluids on each side of it. When the vacuity V disappears, a portion of the second fluid shews itself in the space D *k h*, and it again withdraws itself when the vacuity V touches the sides of the triangular cavity.

In some cavities where there is a large proportion of the second fluid, the vacuities sometimes form *two-thirds* and even *three-fourths* of the space occupied by the expansible fluid when the cavity is full, and yet these vacuities are filled at the usual temperature of 83°. In these cases, the circular vacuity did not contract by heat, but extended itself till it disappeared. This effect admitted of an easy solution, by supposing the surface of the fluid to rise gradually by expansion; but I found, by optical observations, that the vacuity occupied the whole thickness of the cavity, and that it vanished by extension, when it was held in a vertical direction. This remarkable fact will be fully explained in the 5th section.

In some specimens, the faces of the cavities are accidentally inclined to the surfaces, nearly at the angles of total reflection from the surface of the new fluid, so that all the part of the ca-

vity which it occupied appears of a brownish-blue colour, while the part occupied by the second fluid is perfectly transparent. This phenomenon explains, in many cases, the apparent opacity of the cavities, which become perfectly transparent by inclining the specimen. When the stratum of cavities is very much inclined, all of them appear like black specks, and hence they have been generally considered by lapidaries as opaque particles.

Two immiscible fluids, possessing the properties now described, exist also in *Quartz*, *Amethyst*, and *Cymophane*, and I have reason to conclude that the one never occurs without the other, as I have in almost every case discovered the second fluid in cavities, where the difficulties of observation had at first prevented me from detecting it.

Sect. III. *On the Phenomena of Two Immiscible Fluids* without a Vacuity *in the Cavities of Minerals.*

The preceding results conduct us gradually to the development and explanation of phenomena, which, had they been observed alone, would have occasioned no inconsiderable perplexity.

In the same specimen of topaz, I have noticed the two classes of cavities which form the subject of the two preceding sections; and, along with them, I have likewise found a third class, such as AB, Fig. 13., which differs in no respect from those of the first class, shewn in Fig. 1., when examined by the microscope alone. Their difference, however, becomes very manifest by the agency of heat and light.

When heat is applied to these cavities, the circular space N, Fig. 14., in place of diminishing, as it does in Fig. 1., actually increases, as in Fig. 15., as if the fluid WW had contracted with

heat. This perplexing fact induced me to examine the cavity under the circumstances of total reflexion, and it was then apparent, that N was neither a vacuity nor a space filled with gas, but a portion of the new fluid floating as it were on the second fluid WW.

This phenomenon was analogous to what takes place in the right hand portion of the cavity in Fig. 11.; but, as there were here no vacuities into which the expansion of the new fluid could push the second fluid, the difficulty remained unsolved.

It may be proper to mention, that the cavities which present this phenomenon are most frequently connected by a dark line with other cavities, accompanied with vacuities, as shewn at N, in Fig. 16, and 17. In Fig. 16., by a considerable cold, I have caused a small vacuity to appear at V; but it sometimes remains, and sometimes disappears.

As there are cavities, however, such as that in Fig. 14., where no connection can be traced with other cavities, and where the fluid N seems to expand, and WW to contract, it is necessary to seek for some explanation of this singular anomaly. That the expansions and contractions are here only apparent, cannot, we think, be doubted. Let AB, therefore, Plate II. Fig. 18. be a section of the cavity in Fig. 14., where the new fluid NN floats as it were on the other. When NN is heated, the effect of the heat will tend to diminish the cohesive force of the fluid WW, and to make the fluid WW spread itself into a thinner film, as shewn in Fig. 19., so that it seems to occupy a greater space, as shewn in Fig. 15.

In support of this explanation, I may adduce the case of other cavities in topaz, such as those shewn in Figs. 20. and 21., where the globule N of the new fluid never expands with heat,—an effect which is probably owing to its occupying the whole thick-

ness of the cavity, and not a portion of that thickness, as in Fig. 18 *.

With the view of confirming this explanation, I took a cavity AB, Fig. 22., in which the new fluid N occupied the whole of one side of the cavity, and the second fluid W the whole of the other side. Having made the vacuity vanish, and increased the heat to about 200°, the effect of this was to expand N, and make the boundary *a b* move very slowly towards A; but in a short time, a portion of the fluid W, which had thus been pressed out along the bottom of the cavity, made its appearance at the end B, and gradually increased in quantity as *a b* moved towards A. The new fluid then occupied the space between the dotted lines *c d* and *e f*, which contained a greater area than the space between *a b* and B. The portion *e f* B of the second fluid remained for two hours in the position shewn in the figure; but being connected below N with the other portion *c d* A, it was drawn over to the other side, and occupied its original position, as shewn by A *a b*.

In one of the Quebec crystals of *Quartz*, where the cavities are filled with a slightly yellowish fluid, I observed a very deep cavity, such as that shewn in Fig. 23., where the globule N expanded very considerably to the width of *n n* by a considerable heat. I sought in vain for a vacuity, which, however, might have been concealed in a cavity of such a depth, and of such irregularity of surface; but, upon plunging the crystal in hot water, and applying the microscope, I observed two very minute globules, either of vacuity or something else, floating within N, which gradually diminished and disappeared. During another

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* In Figs. 20. and 21. there are small squares, such as S, S, within the cavities, which seem to be filled up with crystallized matter. These squares being sometimes united only by contact with the surface of the cavity, exhibit very brilliantly the colours of thin plates.

experiment with this crystal, one of the cavities burst, with a heat not above 150°, and the fracture round the cavity was covered with specks of an inspissated fluid *.

The formation of cavities with two fluids, and without any vacuity, admits of an easy explanation, when they are connected with other cavities, as in Figs. 15. 16., as there can be no doubt, from the phenomena already described, that part of the fluid W has passed through the narrow channel which connects the cavities. When the cavities, however, are entirely insulated, the explanation is more difficult.

SECT. IV. *On the Changes which these Fluids have undergone in particular Crystals.*

In the absence of all information respecting the nature and constitution of these fluids, it becomes interesting to ascertain, whether time, or accidental causes, have produced any perceptible changes in their physical properties. With this view, I have examined an immense variety of specimens, and have been led to results of considerable interest.

In some specimens of topaz containing the two fluids, I have observed several cavities in which the new fluid N is quite opaque, as at *a*, Fig. 24., and others in which it has the appearance shewn at *b*.

There are some cavities, such as that shewn at *c*, where the fluid seems to have left a crust, lining the interior of it, and there are others where a sort of black farinaceous matter appears, both within and around the cavities, that appear to have been burst by some accidental cause.

* This crystal is the one referred to in page 11.

The most unequivocal proofs, however, of a change in the fluid, are obtained from various topazes, where the induration of the fluid is perfectly obvious to the eye. It resembles a resinous substance, and has a sort of cellular structure, like that shewn at *d*, Fig. 24., where the vacuity retains its circular form. No change whatever is produced upon these appearances by heat. In the figure at *e*, the fluid N, with its vacuity V, still exists, and the latter vanishing with heat; but the induration is distinctly seen at the lower end of the cavity.

In other specimens the same cellular structure appears, but the vacuity has lost, in different degrees, its circular form, as shewn at *f*.

Similar phenomena occur in *cymophane* and *felspar*, in the last of which the induration of the fluid is most distinct.

Sect. V. *On the Vaporisation and Decomposition of the New Fluid at low Temperatures, when enclosed in the Cavities of Minerals.*

Let ABCD, Plate II. Fig. 30. be the summit of a crystallised cavity in topaz, and let the length of the cavity be in a vertical direction, so that SS is the second fluid, NN the new fluid, bounded by a circular line *a b c d*, and V the vacuity in the new fluid, bounded by the circle *e f g h*. Let the face ABCD be placed under a compound microscope, so that the rays of a luminous body incident upon it, may be reflected at an angle less than that of total reflexion. When the observer now looks through the microscope, the temperature of the room being 50°, he will see the second fluid SS shining with a very feeble reflected light, the new fluid NN with a light perceptibly brighter, and the vacuity VV with a light of considerable brilliancy. The boundaries *a b c d*, *e f g h*, are marked by a well defined outline, and also by

concentric coloured rings of thin plates, produced by the extreme thinness of each of the fluids at the edges.

If we now raise the temperature of the room gradually to 58°, we shall observe a brown spot appear in the centre of the vacuity V. This spot marks the visible commencement of evaporation from the new fluid below, and arises from the attenuated vapour which attaches itself to the roof of the cavity. As the heat increases, the brown spot enlarges, and becomes very dark. It is then succeeded by white, and one or more rings rise in the centre of the vacuity. The vapour then seems to form a drop, and all the rings disappear, by retiring to the centre, but only to reappear with new lustre. During the application of heat, the circle *efgh* is in a state of constant contraction and dilatation, like the pupil of the eye when exposed to light, being always greatest when the rings disappear, and contracting its dimensions when they are again formed.

When the vaporisation is so feeble as to shew itself only by a single ring of one or two tints of the second order, they may be made to disappear instantly by the slight degree of heat produced by a single breath upon the crystal; aud the same effect is produced by the approximation of a heated body. When the heat reaches the fluid, however, it makes it throw off fresh vapour, and the rings again appear.

If we put a drop of ether upon the crystal when the rings are in a state of rapid play, the cold occasioned by its evaporation immediately causes them to disappear, till the temperature again rises.

When the temperature is perfectly uniform, the rings remain stationary, and it is interesting to observe the first ring produced by the vapour swelling out to meet the first ring at the margin of the fluid, and sometimes coming so near it, that the darkest parts of both form a broad black band.

As the heat increases, the vacuity V advances to the summit AB, and disappears at $79\frac{1}{2}°$, exhibiting several curious phenomena, which we have not room to describe. One of these, however, is so singular that it deserves to be particularly noticed. After V has disappeared entirely, a brown spot comes from the summit AB, and takes its station in the centre of the ring of the new fluid *a b c d*. This brown tint sometimes rises to higher orders of colours; but disappears by the application of heat. That the coloured rings formed within VV are vapour, and not a film of the fluid itself, may be inferred from its never mixing with the fluid with which it is in immediate contact. It might, however, be a fluid substance, arising either from the decomposition of the fluid itself, or from the condensation of gaseous matter within the vacuity; though this is not very probable, from its constant disappearance when it has accumulated to a certain degree, and its constant reproduction while the temperature remains the same.

These views respecting the vaporisation of the expansible fluid, have been fully confirmed by the discovery of the cavities already noticed, in which the expansible fluid occupies only *one-third* or *one-fourth* of the cavity. Cavities of this kind are represented in Fig. 26., where AB is the cavity, V the vacuity in the expansible fluid *m n o p*, and A *m n*, B *p o* the second fluid. When heat is applied to this cavity, the vacuity V does not contract, as in ordinary cases, but expands, till its circumference coincides with the boundary *m n o p*. This unexpected effect might have arisen from the expansible fluid occupying the lower part of the cavity below V, as in the section, Fig. 27. In this case *c e f d* might have been the vacuity, and the surface of the fluid *e f* might have risen by heat, and gradually filled the vacuity V, while its boundary *c d* retired to *m* and *n* as the surface *e f* ascended. In order to determine if this supposition was true, I placed AB

vertically between two rectangular prisms of glass; and having examined in succession the light reflected from the surfaces *m p* and *n o*, I found that it had suffered total reflexion, both from the side *c d* and the side *g h* of the vacuity, and consequently that the vacuity occupied the whole thickness of the cavity. After the heat was applied, the sides *c d* and *g h* continued equally luminous, and when *c g* and *d h* had retreated to *m n* and *p o*, as shewn in Fig. 28., it became quite manifest, that the space *m n o p* was not filled with the *expanded fluid*, but with the fluid *in the state of vapour*. The coloured rings at first appeared both on the faces *c d* and *g h*, and when the whole was converted into vapour they disappeared, and the light reflected from both the surfaces *m p*, *n o*, which was now uniform, was not that of total reflexion, nor yet that of the expanded fluid, but of an intermediate intensity, corresponding to that of a dense vapour, with a refractive power much lower than 1.21066.

There is another set of phenomena of exquisite beauty to an optical observer, which seem to arise either from the decomposition of the fluid, or the condensation of gaseous matter in the vacuity.

When heat is applied to the cavity, the new fluid has its surface in a state of constant agitation, resembling in the closest manner a surface into which a fluid is discharging itself by drops. When the vacuity is just filled up, one or more drops quit the point where the vacuity disappeared, and pass along the surface of the cavity, like a drop of oil adhering to it in close contact, and never mixing with the fluid. Each of these drops begins in a short time to spread circularly, and to exhibit within its disc an immense number of close coloured rings. By slow cooling the drops become thinner, and the rings less numerous, and more completely displayed, till they entirely disappear at a particular temperature. When the cooling is effected quickly, the

matter which composes the thin plate that exhibits the rings, discharges itself rapidly in gaseous bubbles.

When the drops quit the point where the vacuity vanishes, and pass over one of the summits of the cavity, they often leave an irregular streak, which also gives the colours of thin plates; and sometimes the circular expansion of the drops extends within the circular vacuity, and thus displays two intersecting systems of coloured rings, which proves, in the most incontrovertible manner, that the vapour within the vacuity will not mix with the fluid which composes the drops. The drops now described often quit the vacuity before it is filled up by the expansion of the fluid, and one of them will sometimes remain on the margin of the vacuity, which can be easily seen through it.

Sect. VI. *On the Phenomena of the two New Fluids when taken out of the Cavities.*

From the extreme minuteness of the cavities in topaz, my first attempts to extract the fluid were not attended with much success; but I at last fell upon a method by which I have opened more than a hundred cavities.

When the most expansible of the new fluids first runs from the cavity upon the surface of the topaz, it neither remains still, like the fixed oils, nor disappears, like evaporable fluids. Under the influence, no doubt, of heat and moisture, it is in a state of constant motion, now spreading itself in a thin plate over a large surface, and now contracting itself into a deeper and much less extended drop *. These contractions and extensions are mark-

* A round hemispherical drop often stretches itself into a plane of more than twelve times its original area.

ed by a very beautiful optical phenomenon. When the fluid has extended itself into a thin plate, it ceases to reflect light, like the most attenuated part of the soap-bubble, and when it is again accumulated into a thicker drop, it is covered with all the coloured rings of thin plates. When one of the drops of fluid is very minute and perfectly circular, it resembles, in the most accurate manner, the small drops which pass from the vacuity, and which have been described in the preceding section.

After performing these motions, which sometimes last for ten or twelve minutes, the fluid suddenly disappears, and leaves behind it a residue of minute and separate particles, which are opaque by reflected, but transparent by transmitted light. Upon examining this residue with a single microscope held in the hand, I was surprised to see it again start into a fluid state, and to extend and contract itself as before. This was owing to the moisture of the hand; and I can at any time revive the indurated substance, by the approach of a moist body. A portion of the fluid, which I took out of a cavity twenty days ago, is still capable of being restored to a fluid state by moisture. This portion was shewn to an eminent naturalist, the Reverend Dr Fleming of Flisk, who remarked, that, had he observed it accidentally, he would have ascribed its apparent vitality to the movements of some of the animals of the genus Planaria.

After the cavity has remained open for one or two days, the second fluid comes out of it, and hardens very speedily into a yellowish resinous-looking substance, which is perfectly transparent. This substance absorbs moisture, but with less avidity than the other. It is not volatilized by heat. It is not soluble in water or alcohol; but it is rapidly dissolved with effervescence by the sulphuric acid. The nitric and muriatic acids also dissolve it.

The residue of the first fluid is volatilized by heat; and it is also dissolved, but without effervescence, by the sulphuric, the nitric, and the muriatic acids. After standing some time, both these substances acquire a brilliant lustre, as if some metallic body entered into their composition *.

SECT. VII. *On the Existence of Moveable Crystals in a Fluid Cavity of Quartz.*

Although particles of opaque solid matter have been observed in the cavities of crystals containing fluid, as will be described in the next section, yet, so far as I can find, no crystallized body, and, indeed, no matter capable of crystallization, has ever been discovered in them. The quantities of saline impregnation, indicated by a scarcely perceptible cloudiness in solutions of silver and muriate of barytes, were so minute in Sir HUMPHRY DAVY's experiments, that he considered the water as nearly pure. I was, therefore, in no small degree surprised, when I discovered, in a cavity of a quartz crystal from Quebec, from the cabinet of Mr ALLAN, not only insulated crystals, but a tolerably large group, which were moveable through the fluid upon turning the specimen †. The crystal was perfectly sound round the cavity,

* In opened specimens, which had stood more than a month exposed to the air, I observed small green spheres resting on the surface. They were soft and semi-transparent, like green wax, and varied from $\frac{1}{30}$th to $\frac{1}{200}$th of an inch in diameter. They were not acted upon by any of the above mentioned acids, and were therefore a distinct substance from that of the two new fluids. They occurred in no fewer than 25 out of 40 crystals, *three* being sometimes found in one specimen; and there can be no doubt that they consisted of fluid matter which had oozed out of the crevices of the mineral.

† There were also numerous opaque particles in the cavity, which descended slowly in the fluid.

which had a sort of triangular form, one of the sides of the triangle being about one-tenth of an inch long. The fluid was quite transparent; and, as the air-bubble was not perceptibly diminished by heat, there is every reason to think that the fluid is water. The crystals were transparent to a considerable degree, and had a white milky tint, when seen by reflected light.

In considering the circumstances of this singular phenomenon, we are led to suppose, that the included crystals had been dissolved in the fluid at the time of its being shut up in the quartz, and had afterwards been deposited from the solution. The ingenious supposition of Sir HUMPHRY DAVY, that a liquid hydrate of silica may exist at high temperatures, and may contain small quantities of atmospheric air, will no doubt explain the phenomena of water in rock-crystals; but it is not easy to comprehend how the formation of a group of crystals could either have accompanied or followed the separation of the water and the silex.

As the specimen now alluded to is too valuable to be destroyed, for the purpose of analysing the minute crystals, it is probable, that our information respecting them would have been very limited, had not a circumstance of an accidental nature enabled me to throw some farther light on the subject. Several years ago, when I was examining, along with Earl COMPTON, a large collection of quartz crystals from Quebec, for the purpose of obtaining remarkable crystallizations, I was much struck with the appearance of several spherical groups of whitish crystals, within some of the specimens. Upon pointing out to Lord COMPTON this peculiarity, his Lordship agreed with me in thinking that they belonged to the Zeolite Family. Having purchased all the specimens that could be found, I have since repeatedly examined the included crystals, with the view of determining their nature. I found that they did not belong to the zeolites, but consisted principally of carbonate of lime; and, as

every mineralogist who saw them considered them as something new in appearance, I expected that a greater quantity of them might be found for the purposes of analysis. Familiarised, therefore, with the aspect of these groups, I was convinced that the crystals in the fluid cavity were the same substance; and a more accurate examination has established their perfect identity.

These white crystals sometimes occur in minute insulated spiculæ within the solid mass, but most frequently in spherical groups of extreme beauty, surrounded with the most transparent quartz. Many of the open hollows and crevices of the quartz crystals are filled with them, and numerous aggregated groups adhere to their external surface. These crystals, though very minute, I have found to have a powerful double refraction; and as they are wholly dissolved with effervescence, excepting a little adhering silex, in diluted nitric acid, there can be no doubt that the external crystals and consequently those in the fluid cavity, are *carbonate of lime* *.

Sect. VIII. *On the Phenomena of a single Fluid in the Cavities of Minerals and Artificial Crystals.*

The phenomena which I propose now to describe, are essentially different from those which form the subject of the preceding sections. The fluid which occupies this class of cavities exhibits no properties different from water or mineral oil, which have long ago been detected by mineralogists, and the vacuity which often accompanies these fluids, is either a perfect vacuum, or filled with a gaseous body.

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* Since these observations were made, Mr Nordenskjold has confirmed this result by experiments made with the blowpipe.

This class of cavities might, with propriety, have been divided into three subdivisions : 1. Those where the cavities are entirely filled with fluid ; 2. Those which have a perfect vacuum along with the fluid ; and, 3. Those where the fluid is accompanied with a gaseous body ; but, as several crystals seem to possess cavities with all these characters, I shall describe the different crystals in their order.

1. Amethyst from Ceylon.—This fine specimen, in the cabinet of Mr Thomson of Forth Street, originally belonged to the King of Candy. It is about 3 inches long and $1\frac{3}{4}$ broad, and has a large cavity, of the size and form shewn in Fig. 29. The bubble V, which I have ascertained to be gaseous, by the reflexion of light, moves by starts from one end of the cavity to the other. It is not sensibly altered by heat. Another cavity C, near the large one, has a small air-bubble in the middle, which refuses to move from its place. There are several pieces of opaque solid mater, which, with a little management, may be seen within the cavity AB, and which may be made to fall from one side of it to the other. This is the largest cavity that I have ever seen in a solid crystal.

2. Rock Crystal.—This mineral abounds with cavities, containing water and mineral oil, which is sometimes black, sometimes of a faint yellow, and sometimes of a rich orange red colour.

The largest cavities are generally amorphous ; but there are many crystals with thousands of cavities all regularly crystallized, and of the exact form of the secondary crystal.

The quartz crystals from Quebec contain great quantities of mineral oil, which does not perceptibly expand by the application of heat. There are frequently within the cavities dark little fragments, which are carried about by the motion of the

fluid. In a crystal of quartz belonging to Mr Allan, and containing a large cavity, with water and an air-bubble, he observed a little black globule which adhered to the air-bubble. Upon looking at it afterwards, he remarked that the black globule had separated into a great number of minute black particles. This opaque matter is likely to have had the same origin as that which is described in page 23.

One of the most remarkable specimens of quartz which I have ever met with, was shewn to me by Mr Sivright. The cavities are of the most singular shape, and are almost all nearly filled with a fluid, accompanied with a small air-bubble, which does not perceptibly expand with heat. Some of the cavities contain a yellow fluid, with various air-bubbles, which seem to be naphtha apparently in a very viscid state. This specimen is shewn, though very imperfectly, in Fig. 30.

3. Topaz.—There are many topazes from Brazil, New Holland, and Scotland, which contain a single fluid, with an air-bubble. In these the fluid does not perceptibly expand with heat; and I have ascertained that it is aqueous, and that the vacuity is filled with a gas.

In several topazes, both from Aberdeenshire and Brazil, the form of the cavities is extremely curious, resembling the writing in Eastern MSS. These grotesque forms generally contain the new fluid; but many of them have no vacuity at all, while some of them contain a fluid of a decided yellow colour, which I have never found accompanied with a vacuity. These cavities are shewn in Fig. 31. The cavities in topaz containing the two new fluids are shewn in Fig. 40.

In a particular specimen of topaz, I observed a regular rhomboidal space apparently filled with particles of dust suspended in it. This rhomboidal space appeared *green* by reflected, and *red* by transmitted, light.

4. Cymophane.—In several specimens of cymophane, there are strata of cavities apparently containing one fluid, but without any perceptible vacuity. In the crystal containing the stratum with the new fluids, there is another stratum parallel to it, of a very remarkable kind, where the cavities have the form shewn in Fig. 32. The nature of the fluid, however, I have not been able to determine.

5. Peridot.—The largest and finest crystals of this mineral are often intersected, in various directions, with strata of fluid cavities having globules of air. In a set of unusually large crystals a kind of resinous indurated matter seems to have been diffused, sometimes in strata and sometimes throughout the mass of the crystal. These peridots, which are very magnificent, belong to the Countess of Wemyss; but in consequence of their being cut and set in gold, it was impossible to subject them to an accurate examination.

6. Felspar.—The cavities in this mineral are very flat, and irregularly formed. They contain a single fluid and an air-bubble, which neither vanishes nor diminishes with heat.

7. Emerald and Beryl.—The great degree of foulness which is so common in these gems, arises generally from strata of cavities containing a single fluid, and an air-bubble, which do not perceptibly decrease with a temperature of 150°.

8 Fluor-Spar.—The crystals of green fluor-spar from Alston Moor frequently contain cavities with water. I have seen several about half an inch long, and of the form of a triangular pyramid. The air-bubble moves sluggishly even in these large ones, and with great difficulty in the small ones. The apparent air-bubble is gaseous, and the fluid does not perceptibly expand

with heat. These crystals frequently burst with a heat not above 150°. In several cavities I have observed solid fragments falling through the fluid, by the inversion of the crystal.

9. Sulphate of Lime.—In this mineral the cavities have often a very singular form; and in all the specimens which I have examined, the fluid is aqueous, and is accompanied with a gas or a perfect vacuum.

In Fig. 33. I have represented one of the most singular arrangements of cavities that I have met with. In order to determine the thickness of the cavity, I reduced the specimen, so as to give the polarized colours of the second order of Newton's Scale, and, by carefully observing the difference of tint in the cavities, and in the solid parts, I obtained a measure of the thickness of crystalline matter abstracted in these parts. The difference of tint was very obvious, and proved that the thickness of the cavity did not exceed the $\frac{1}{5000}$th part of an inch.

In many specimens, these very shallow cavities occur in long canals. In others they resemble some foreign crystalline matter, shooting out into the most singular forms, as at *a*, *b*, Fig. 34. and sometimes the cavities appear to the eye like tufts of white silk compressed between the laminæ, though they are, in reality, strata of rhomboidal cavities, occurring in thousands, and arranged in the direction of their longest diagonals, while the strata themselves are highly inclined to the surfaces of the laminæ. In other specimens, the cavities have the most singular forms, as represented in Fig. 34. One of the canals in sulphate of lime is shewn at AB, in Fig. 35., where *a b c d*, *e f g h* are two air-bubbles or vacuities. By applying heat to the side B, these air-bubbles shift their places. All the lines *a b*, *c d*, *e f*, *g h*, advance to B, but *c d* and *e f* approach to one another, and the moment they come in contact, the two vacuities are converted into one, which has the position $a' \; b' \; g' \; h'$.

10. SULPHATE OF BARYTES.—The cavities in sulphate of barytes were first pointed out to me by Mr SIVRIGHT. I have since found them in various specimens. They are generally of a very irregular shape, though sometimes they have regular crystalline forms.

Many of these cavities are entirely filled with fluid, but in several a very small apparent air-bubble may be seen. This vacuity does not vanish by the heat of the hand, but it disappears entirely at a temperature of about 150°, and again returns when the specimen has cooled. It is therefore a vacuum.

11. CALCAREOUS SPAR.—Cavities filled with water are frequently found in calcareous spar. The apparent air-bubbles, which are very small, occur only in some of the cavities. To some of the cavities I applied a heat of about 150°. The apparent air-bubbles entirely vanished, and, what is very remarkable, they have never again reappeared. This singular fact may be ascribed to the strong cohesion between the fluid and the sides of the cavity, which can only be overcome by a greater degree of cold producing a greater degree of contraction than that of the cabinet in which the specimen has been kept.

12. ROCK-SALT FROM CHESHIRE.—In this mineral the cavities assume the most beautiful forms. In one specimen, shewn in Fig. 36., they have the form of regular cubes of various sizes, and with numerous truncations on their sides and angles. In other specimens the cavities have the form of octohedrons; while in others they have numerous varieties of forms. The cubical hollows above mentioned are in general perfectly filled with fluid; but some of them have small apparent air-bubbles, which contract to fully one-third of their size by a heat of 120°.

13. Sulphate of Iron.—The cavities in this salt are sometimes finely crystallized in the form of prisms, with double pyramids, and the sharpest truncations. In the same specimen they are frequently oval, or imperfectly spherical. They sometimes contain apparent air-bubbles, and are often quite filled with fluid. By the application of heat, these vacuities disappear entirely, and reappear by cooling.

14. Sulphate of Nickel.—In this salt the cavities are sometimes amorphous, and sometimes beautifully crystallized. These vacuities frequently disappear by heat, and reappear by the application of cold.

15. Sulphate of Copper.—The air-bubbles move about in this salt by the application of heat, but never vanish. By increasing the heat, they diminish a little in size.

16. Alum.—The air-bubbles in alum do not perceptibly change their magnitude by heat. I have opened several cavities in this salt, but have never found the air to be either in a state of dilatation or compression. The fluid seems to be pure water.

17. Tartrate of Potash and Soda.—The cavities in this salt are both crystallized and amorphous. A considerable vacuity in a large cavity vanished completely with heat; and in others, where the vacuity was very large, it became extremely small when heated. Two separate cavities, with separate vacuities, became one, and united their vacuities. These phenomena, no doubt, arose from the fluid having its dissolving power increased by heat; and it is probable that the disappearance of the large vacuity arose from the dissolved salt occupying more space in its fluid than in its solid state.

It is unnecessary to extend these details to all the artificial crystals enumerated at the beginning of this paper, as I have not observed in them any phenomena different from those which have already been described.

There is another class of cavities which require to be studied with some attention, namely, those which are entirely full of fluid, or entirely empty. Mr Sivright first observed cavities in the diamond *, and in garnet; but, though I have examined them in various specimens, I have not been able to determine whether they are entirely filled with fluid, or are entirely empty. I have found cavities of a similar kind in *cinnamon-stone*, where they are beautifully crystallized, in *sulphate of Strontian*, in *sulphur*, in *analcime*, and in *chabasie;* but I observed no appearance of air-bubbles, and have no certain evidence that they contain a fluid †.

It would be improper to conclude this paper, without noticing the relations which are supposed to subsist between this class of phenomena and the two contending Geological Theories. The existence of highly rarified gas in the cavities of crystals, has been regarded by the distinguished President of the Royal Society of London, as " seeming to afford a decisive argument in favour of the igneous origin of crystalline rocks ;" and the " fact of almost a perfect vacuum existing in a cavity containing an expansible but difficultly volatile substance," (as naphtha), he likewise considers as highly favourable to the same theory. The discovery of compressed gas in similar cavities might have been regarded as neutralizing, in some degree, the first of these argu-

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* See the *Edinburgh Philosophical Journal*, vol. iii. p. 98., for an account of the polarising structure which sometimes exists round the cavities in diamond.

† This point may be easily determined by grinding the specimens, and examining the light reflected at the surfaces of the cavities.

ments: but Sir Humphry Davy remarks, that it may be explained by supposing the crystal to have been formed under a compression much more than adequate to compensate for the expansive effects of heat *.

Without presuming to combat these deductions, or to suggest any of the numerous explanations by which the Neptunist might reconcile with his own system the compressed and dilated condition of the included air, I shall content myself with stating, that the facts described in the preceding paper appear to me decidedly hostile to the igneous origin of crystals, and, in some points of view, favourable to their aqueous formation. The existence of a fluid which entirely fills the cavities of crystals, at a temperature varying from 74° to 84°, may, upon the principles assumed in the opposite argument, be held as a proof that these crystals were formed at the ordinary temperature of the atmosphere, while the fact of a perfect vacuity existing in *sulphate of barytes*, and capable of being filled up by the expansion of the aqueous fluid, at a temperature not exceeding 150°, authorises the analogous conclusion, that the crystal could not have been formed at a higher temperature. On the other hand, the filling up of the vacuities in *sulphate of iron*, and *sulphate of nickel*, at a temperature much above that at which they were formed, may lead geologists to renounce a species of argument which appeals only to our ignorance, and to withdraw from the defence even of their outworks, those faithless auxiliaries, which are so ready to enlist themselves in the service of either power.

There is one geological relation, however, of the preceding facts, which may deserve some attention. Hitherto the contend-

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* As the effects of heat and compression might exactly balance each other, the gas would in this case be atmospheric air, in a common state of density; so that the volcanists are here sheltered against experimental hostilities, amid the generalities of their hypothesis.

ing theorists have limited their idolatry to two of the elements; but the existence of two new substances in minerals, one of which combines a great degree of fluidity with the high expansive power of the gases, renders it probable, either that these substances existed at the formation of the globe, or that they are the result of laws of crystallographic combination which have escaped the notice of the philosophical geologist. Were such fluids the product of the ordinary processes of crystallization, they would occur in artificial as well as in natural crystals: and, consequently, while they remain undiscovered in the cavities of the first of these classes of bodies, we are entitled to attach a new difficulty to the aqueous hypothesis.

Had the two new fluids occurred only in one mineral, or in minerals of a particular composition, they might have been supposed to have some relation to the elementary principles of the body, and to have arisen either from some accidental irregularity, which prevented them from crystallizing, or from the decomposition of the matter subsequently to its crystallization. The perfect identity, however, of the two fluids, as found in pure Quartz, in Amethyst, in Topaz, and in Cymophane,—minerals brought from the most opposite parts of the globe,—from Scotland, Siberia, New Holland, Canada, and Brasil,—establishes the universality of their existence, and adds to the probability of the supposition, that they have performed some important function in the organization of the mineral world.

While the preceding facts thus obviously connect themselves with our geological theories, they promise also to be of some use in the practical branches of Physics. A fluid possessing such a high expansive power would be invaluable in the construction of delicate Thermometers, and various other philosophical instruments; while its extreme fluidity would enable us to construct levels of singular delicacy. If the resources of the chemist shall

not enable him to form such a substance, a plate of topaz, with particular longitudinal cavities, might be used, as a delicate thermometer, for certain ranges of temperature; and where slight variations require to be observed, several such plates might be of essential service in many researches, both of a chemical and a physical nature.

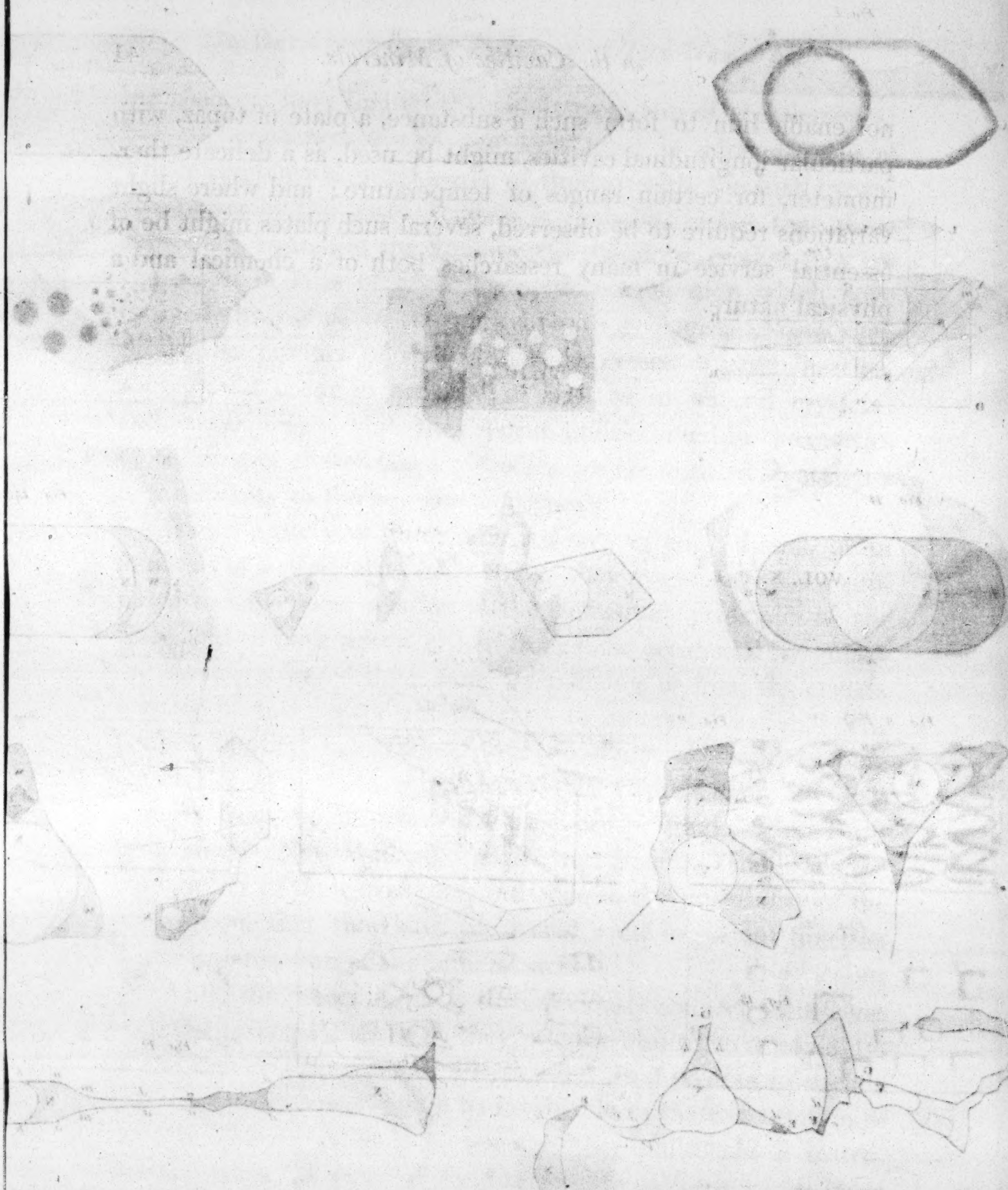
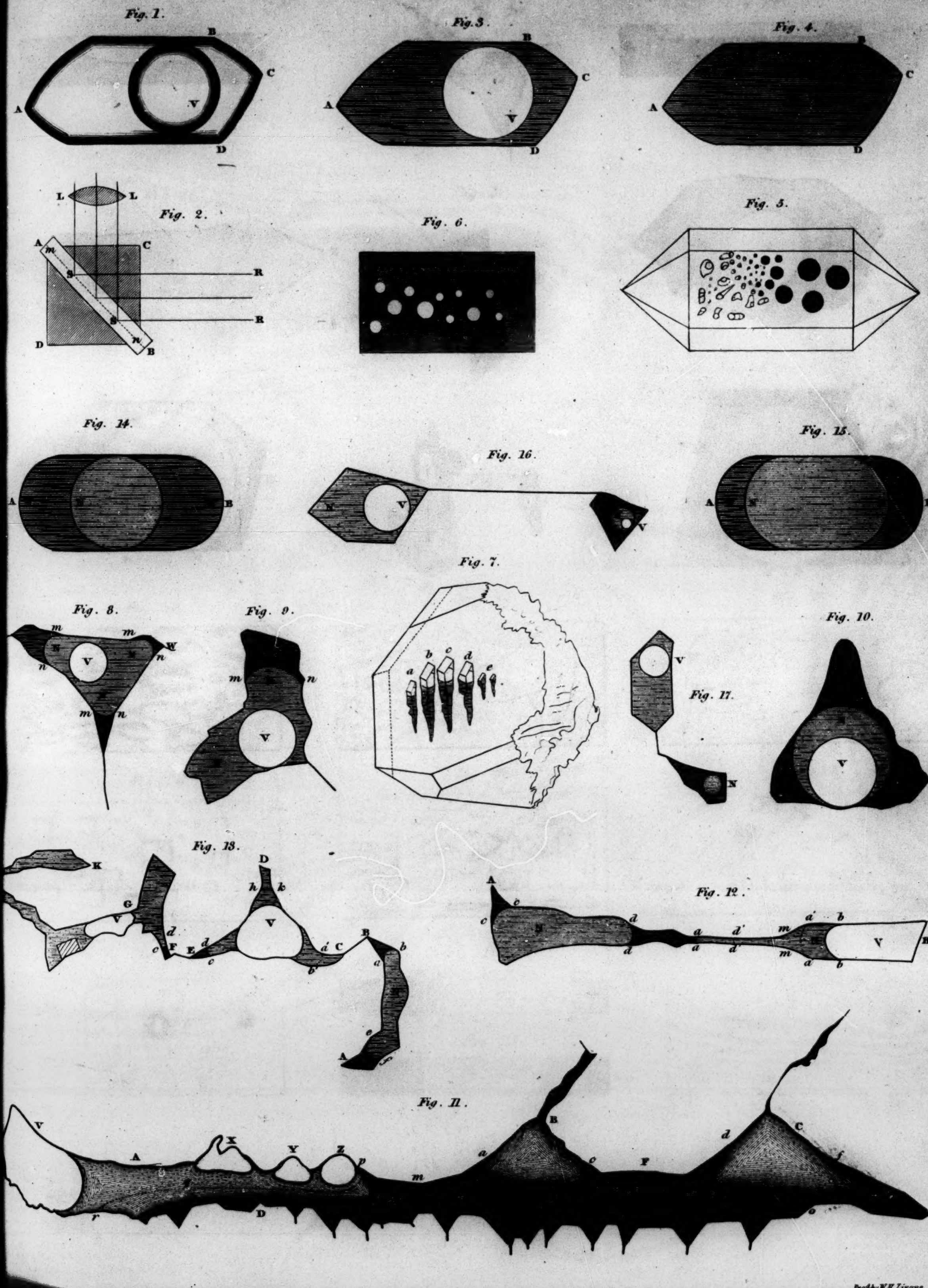


PLATE. I.

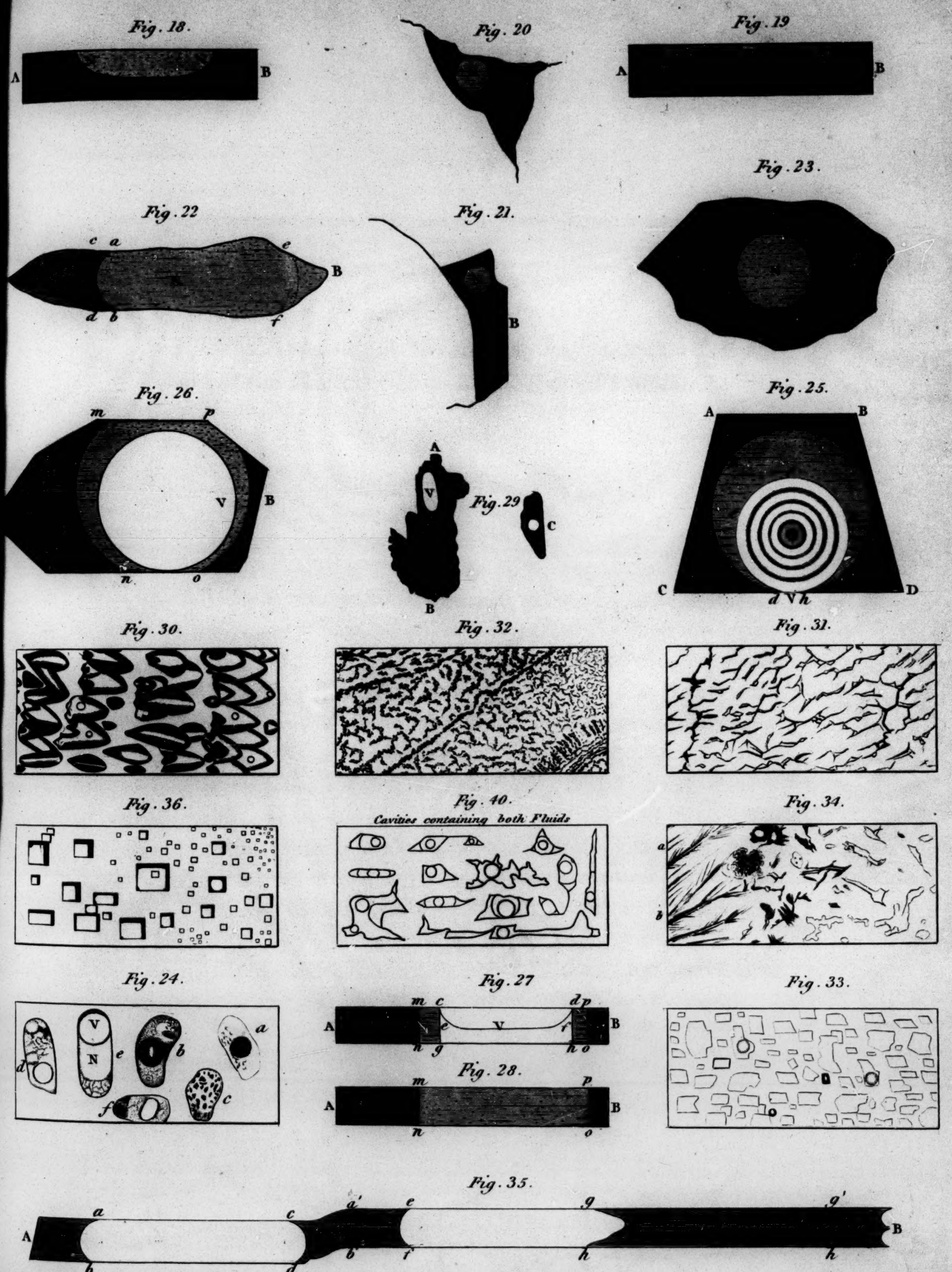
Eng.d for the Royal Soc. Trans. Vol. X. page 42.



Eng.d by W.H. Lizars.

PLATE II.

Engd. for the Royal Soc. Trans. Vol. X. page 42.



Engd. by W. H. Lizars

II. *Observations on the Comparative Anatomy of the Eye.* By Robert Knox, M. D. Member of the Wernerian Society, and of the Medical Chirurgical Society of Edinburgh.

(*Read June* 17. 1823.)

The following observations, which I have the honour to present to the Society, have arisen out of an inquiry into the structure and distribution of the nervous system throughout the animal creation. It will readily be imagined, that the nerves supplied to the organs of sense, did not fail strongly to attract my attention, and that those belonging to so important an organ as the eye, were considered by me as worthy of the most minute investigation. It was impossible to proceed in this inquiry without submitting the organ itself to a very careful examination, in executing which, several important facts presented themselves, which I believed to be novel, or, at least, to lead to views respecting the physiology of the eye, different from those generally adopted. It became my duty to search into authors of the present and of the past age, and to collect into view whatever had been previously written on the subject; but leisure being altogether wanting for such an undertaking, I have thought it best to describe what I have myself seen.

F 2

I. *General Idea of Vision.*

It has been said by a distinguished physiologist, that we cannot obtain an immediate knowledge of real distance by sight alone. Our countryman, Dr Porterfield, however, demonstrated that, within certain limits, perhaps within the range of distinct vision, our knowledge of distance is perfect, and that it depends on the organ being double and symmetrical. Beyond this point, the mode by which we judge of distance becomes complex, for we have to avail ourselves of other senses, and more particularly of that of feeling. We reason also respecting the distance of objects, from their degrees of illumination, from their obscuring each other, from their magnitude, &c. With all these aids, many individuals judge very erroneously of distance, whilst others decide upon it with the utmost precision. This faculty depends, therefore, on a peculiarly organised brain, conjoined with much experience. The superiority of one military genius over another is in no small degree attributable to this talent. History is filled with the errors of those, who, miscalculating distance, have either crowded their troops into a space unable to contain them, or, by too great an extension of their lines, have presented a feeble front, incapable of resisting the enemy. Such errors must necessarily lead to serious disasters.

A particular eye sees distinctly objects placed at only a certain distance, hence men are either short or long sighted; but we know that experience exerts great influence over this. Even the vulture, whose eye is beyond all doubt the most keen and piercing, is conducted by experience to a knowledge of the prey

suited to him *. I have repeatedly seen this bird deceived, and have ascertained on many occasions, that though birds possess the power of very distant vision, such vision is not distinct. By the point of distinct vision, I understand the distance at which a moderately small object may be clearly and distinctly made out, independently of all experience. It must be obvious to every one, that this distance is very limited, and depends, to a certain extent, on the size of the object. Instead, therefore, of talking of the limits of distinct vision, we shall consider the faculty which the eye possesses, of adapting itself to the general perception of objects placed at a variety of distances. We shall return to this subject, when describing the means employed in effecting the changes supposed to take place within the eye-ball.

There is still one other faculty possessed in various degrees of excellence by different individuals; I allude to rapidity of vision, or the power of instantaneously changing the focus of the eye, and adapting it to objects placed at various distances. This, no doubt, depends on the irritability of the organ, or, perhaps, of the nervous system; it is connected, as shall be shewn, with the distribution of the ciliary nerves, and is of the same nature with muscular motion in other parts of the body; it is not peculiar to any complexion, and hence will be found to be as commonly possessed by the blue as by the dark eye. The exercise of this faculty to its greatest extent, is what constitutes the sportsman; but most men possess it to a considerable extent when roused by danger, or excited by the unexpected appearance of an extraordinary object. I have observed remarkable differences amongst individuals in this respect, and shall cite an

* I have demonstrated, in a brief notice, which, I believe, was inserted in some of the French journals, that it is by sight only that the vulture is led to discover his prey, and not by the sense of smelling.

instance, which occurred in the person of a friend, whose powers of vision were uncommonly strong, but whose eye, which was dark, was so ill adapted to effect a sudden alteration of the focus, that, on hunting parties, he was always the last to discover the game. It is extremely probable, that a principal part of the phenomena depends on the rapidity of motion in the iris. I think that I have noticed it oftenest in those in whom the iris was of a blue colour.

The same individual may, with some attention, distinguish the same object at different distances, the limits of which may be assigned, with respect to each individual. The eye, therefore, must have the power of changing the position of its parts, by some means or other, and these must be placed, either within it, or exteriorly to it. So far as I am aware, no satisfactory theory has been offered concerning this accommodating power of the eye, nor has any one fixed on the parts by which the internal changes are effected. It is true, that in viewing near objects, the pupil admits only the rays which are nearest to the axis, and which are consequently the least diverging. This has been supposed by some to be capable of accounting for the phenomena; but we shall, in the sequel, endeavour to shew, that this mode of accounting for our power of viewing very near objects, though it be essentially requisite to an accurate and complete theory of vision, is yet totally inadequate to account for the whole of the phenomena. This was the opinion of the late JOHN HUNTER, very few of whose assertions have ever been confuted.

It has been suggested by the celebrated Baron CUVIER, that perhaps the limits of distinct vision are much more confined than we imagine them to be; and, it is probable, that, in many cases, it only appears distinct, because it is assisted by the recollection which we have of the object. Now, this should be constantly borne in mind, for it prevents us from entertaining exaggerated notions of the power of vision possessed by a variety of animals.

Of the Figure of the Globe of the Eye; of the Form and Proportions of its Chambers, and of the Density of its Transparent Parts.

I have but little to add from personal observation, to the labours of preceding writers on this subject. Indeed I suspect that little remains to be done relative to the matter announced in the section. So important an organ is that of sight, that it has at all times attracted the attention of the profoundest philosophers, and the examination of its various parts has been deemed not unworthy the labours of the greatest mathematicians of the present age. To the splendid works of these gentlemen I beg to refer the Society.

The anterior part of the eye in fishes, and in the cetacea, is flat. In the cuttle-fish, the cornea and aqueous humour are wanting. In man, and in quadrupeds, it is almost spherical. The cornea makes a slight projection anteriorly, because its convexity is a portion of a sphere smaller than that of the rest of the eye. In the porcupine and the opossum, this is said not to be apparent, and it seems to me that the same remark is nearly applicable to the ornithorinchus paradoxus. In birds, the cornea is exceedingly convex, and is sometimes completely hemispherical. Thus we see, that the difference in the eyes of these animals is connected with the proportional density of the media in which they live. It is true that the ostrich and cassowary never rise from the ground, and yet have their eyes formed like those of other birds; but as Nature has formed the various classes of animals agreeably to certain determinate laws, and as in none of these classes has she adhered so strictly to these laws as in birds, so we can see no good reason why the eye of the ostrich should differ much from that of other birds; or perhaps it

might be better stated, that as the eyes of the ostrich and cassowary have a strictly ornithological character, this fact may be offered as an instance of the close observance which Nature pays to her general laws. I have thought that the vitreous humour in birds was less dense than in the mammalia, but I have no direct experiments to prove this.

Of the Sclerotic and Transparent Cornea.

The sclerotic, or external covering of the eye-ball, is so intimately connected with the dura mater, forming the sheath of the optic nerve, that it may be considered, as in some measure, an analogous membrane. Anteriorly, its connection with the cornea presents a variety of forms, but there is this uniformity in all the animals which I have examined, namely, that the internal layer of the cornea, that to which the name of the Tunic of the aqueous humour has been given, does not unite with the sclerotic, but with the iris. The mode in which this union takes place is simply this: the whole external layer of the sclerotic passes forwards beyond the *circulus niger*, and is inserted into the edge of the cornea. The inner membrane of the sclerotic, of the origin of which I shall presently speak, in its passage forwards, is interrupted by that portion of the *annulus albus* which is left adhering to the sclerotic, when the choroid has been forcibly removed from it, but may be readily detected between this portion of the annulus albus, and the posterior edge of the membrane of the aqueous humour, to which, it seems to me, to attach itself. Whether this inner membrane of the sclerotic, and that of the aqueous humour, be really the one a continuation of the other or not, I have, as yet, been unable satisfactorily to make out. I am inclined to think that they are attached and not continuous membranes, though an appearance supporting the

latter opinion has been observed in the eye of the lion. In birds and fishes, a reflected portion of the membrane of the aqueous humour seems to cover the whole anterior surface of the iris, and is the cause of that peculiar elasticity observable in the iris of birds *, by which the relation of the iris to the cornea is changed, when life is extinct, from a right angle to a very acute one. In the eye of the deer, the ox, &c. the anterior layer of the iris is connected to the inner membrane of the cornea, by numerous short and seemingly tendinous fibres, and these membranes are evidently distinct.

The inner membrane of the sclerotic is simply a reflected membrane of the choroid. Near to the origin of the retina, or at least to its point of union with the optic nerve, the inner membrane of the sclerotic can hardly be distinguished from the choroid, which may here be divided into as many layers as the anatomist chuses. It is curious enough to trace the use which anatomists, from preconceived notions, have been induced to make of the pia mater. The celebrated ZINN and WRISBERG considered the inner layer of the sclerotic to be a continuation of the pia mater; others, equally celebrated, imagined the outer layer of the choroid to be the true continuation of the cerebral membrane. It was soon transferred to the inner layer of the choroid; and, lastly, to the membrane immediately in contact with the retina, and which has lately been described by Dr JACOB in the Philosophical Transactions. This latter opinion was held by the late Dr MONRO. I do not consider the inquiry as being of the least importance.

From the flexible nature of the sclerotic coat in man and quadrupeds, and even in birds, it has been supposed that compression exercised upon it by the muscles of the eye-ball, may

* This is not present in the eye of the cassowary.

swell the cornea, by pushing the humours forward, and thus enable the eye to distinguish very near objects. But this theory, though supported by so distinguished an anatomist as BLUMENBACH, is by most deemed inadmissible. It appears to me truly wonderful, that such a theory could have stood its ground for any length of time; for, if it was intended by it to explain the great power of vision in birds, it was unnecessary, since the natural form of the eye-ball, and of its contained parts, is in them sufficient to explain the phenomenon; or, if proposed as a theory, by which to explain the accommodating powers of the eye, it was not less in fault, since, in man, the changes produced in the cornea, at whatever distance an object be placed, are altogether trifling, and nearly inappreciable; nor have I observed the least alteration in the cornea of birds, whatever might be the distance of the object they viewed. Lastly, Compression of the eye-ball does not cause a protrusion of the cornea, though this experiment be made with the eye of the horse, whose sclerotic is comparatively thin and flexible. During life, the cornea is perpetually tense, clear, and, as it were, sparkling, especially in the young and healthy. This is owing to the contraction of the external muscles of the eye-ball, an action which persists with life, and is named by physiologists the tonic power of the muscles, in contradistinction to the action caused by volition. After death these muscles relax, the eye-ball is left to itself, and the cornea becomes flat and dim. These appearances have generally been attributed to the escape of the aqueous humour through the cornea, now deprived of life; but this is a gratuitous and unnecessary supposition. It was long ago remarked by Dr WHYTT *, that, in apoplexies, whilst the patient still lived, the eyes lost their lustre, and, in some cases, put on the appearance of those

* See his Physiological Essays.

of a person actually dead. We have, moreover, seen many instances, in which the cornea retained its fullness and lustre for some time after death had taken place. These various effects are attributable to the different degrees of energy remaining in the external muscles of the eye-ball. After a time, a film forms on the surface of the cornea. I have already stated, that the anterior layer of the iris is inserted into the inner membrane of the cornea, and that this forms a principal attachment. If the anatomy of the eye in fishes be considered as affording a fair analogy, we are warranted in asserting, that the inner membrane of the cornea covers the whole anterior surface of the iris, though the ox, the deer, and some other animals, present the modification before described. On this depends the elasticity of the iris in birds; and, in fishes, we are compelled to admit this continuity of the membranes; for, between the coloured portion of the iris, which, in them, we know to be the external layer of the choroid, and the aqueous humour of the anterior chamber, there exists no other membrane, than a thin, transparent, elastic tunic, continuous with, and exactly resembling, the inner layer of the cornea. We cannot well doubt that the action of the iris on this membrane must alter, to a certain degree, the form of the cornea internally, and, consequently, that of the aqueous humour, which opinion was long ago maintained by Jurin, and admits almost of demonstration.

II. *Of the Choroides, and its Appendages; of the Iris and its Motions; of the entry of the Optic Nerve into the Eye; and of the Distribution of the Ciliary Nerves.*

We give the name of Choroides to that dark coloured membrane found immediately within the Sclerotic. By Comparative Anatomy, we best learn the nature of this membrane, the num-

ber of its component tunics, &c. Near its commencement in the bottom of the eye-ball, it adheres very intimately to the inner layer of the sclerotic, the one being simply a reflexion from the other. Advancing forwards this union ceases, and they adhere only at those points where vessels and nerves pass from without to the choroides. In some animals, these pass directly through towards the choroid, but in birds and the deer tribe their course is oblique. We shall return to this whilst describing the peculiarities in the anatomy of the eye of the deer. Still further forward, they are more intimately united by the annulus albus, and here the external layer of the choroid is supposed to terminate. I have reason to think, that, in general, it does not terminate, but passes forward with the inner layers of the choroid, to form the ciliary processes and uvea. In fishes, where the annulus albus is quite rudimentary, and does not impede the passage of the choroid, or render its termination obscure and complex, the external membrane of the choroid is observed to pass onward to the edge of the pupil, nor can the most careful dissection, aided by good glasses, demonstrate any additional tunic to exist between it and the transparent covering it receives from the cornea. In some of the mammalia, and in birds, it has appeared to me, that the external layer of the choroid inincludes the annulus albus in part, or perhaps rather that it becomes looser in texture, and unites with the inner layer of the sclerotic, and so passes forward towards the cornea : in general, however, it seems to unite with the inner layer of the choroid, and to pass forward towards the uvea internal to the annulus albus. When the sclerotic and cornea are carefully removed in the eye of any of the mammalia, the parts seen, and which we may mention in succession, are, the exterior surface of the iris ; the line by which this exterior surface of the iris, near its base, is naturally connected with the cornea, and which anatomists have called the Circulus niger ; a dark coloured membrane, connecting

PLATE III.

Fig. 1.

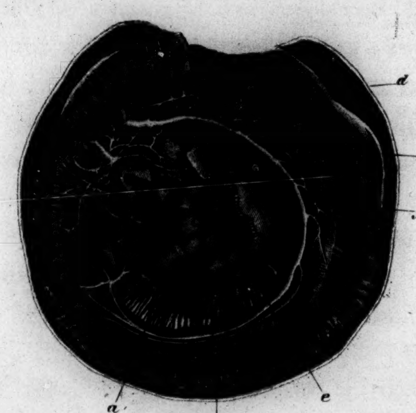


Fig. 4.

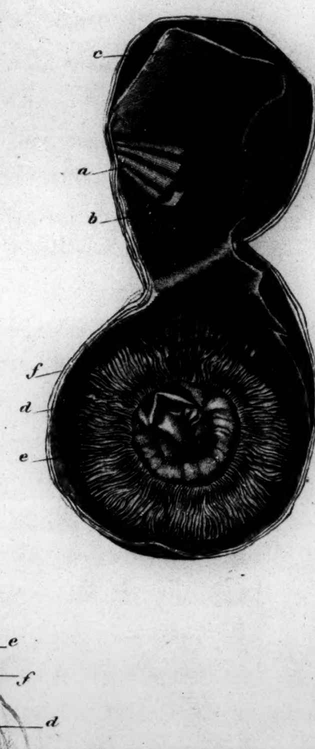
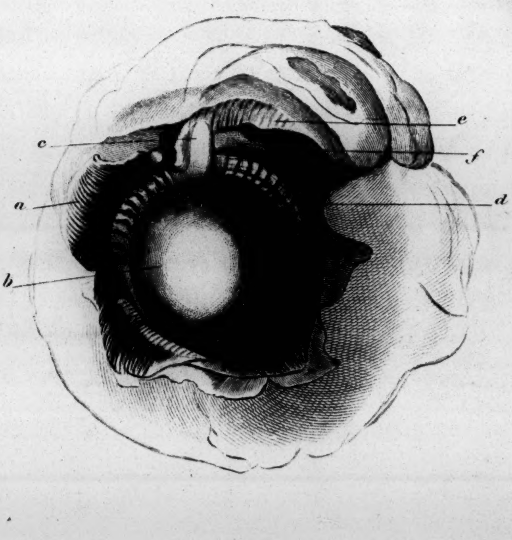


Fig. 3.

Fig. 2.



Lizars Sculpt.

the base of the iris with the anterior edge of the annulus albus, and which may either be considered as a peculiar body, or a portion of the choroid tunic immediately subjacent: this dark coloured body forms the inner surface of a cavity, which will be more minutely described afterwards: lastly, the annulus albus itself connected posteriorly with the outer layer of the choroid coat. The anatomy of these parts shall be described more minutely whilst speaking of the various classes of Animals.

As it is in birds that some of these parts are most distinctly and readily made out, I shall here briefly describe the general anatomy, drawn from very numerous specimens, of almost all the natural families of that class; but I shall dwell more particularly on the eye of the cassowary, not because there is in it any peculiarity, but because the magnitude of the eye-ball permits the minutest parts to be satisfactorily demonstrated. Near the posterior edge of the annulus albus, the choroid is firmly fixed all round the eye-ball to the inner layer of the sclerotic. At this point, the external layer of the choroid may be considered either as terminating, or what, in favourable specimens, may be partly demonstrated, as passing forward united to the inner layer, to form the ciliary body. Anterior to this is another adhesion, between the anterior expansion of the annulus albus and the sclerotic; this adhesion may be supposed to proceed onwards, and to terminate in the cornea by a thin broad membrane, generally of a dark colour.

Anterior to the principal insertion of the annulus albus into the sclerotic, and lying on a strong membranous expansion, stretching between the annulus albus and cornea, are found the ciliary nerves, which here resemble a plexus, being broad, large, and ribbon-like; they completely surround the eye-ball, and send numerous distinct branches to the iris and to the annulus albus *. I had already remarked, during the course of my dissec-

* See Plate III. Fig. 1.

tions, that the annulus albus assumed a variety of appearances, but that it resembled a ligament in a very few animals only: that in still fewer did it bear any resemblance to a nervous ganglion; which resemblance, I speedily satisfied myself, was a complete deception. At last, having discovered, that in birds, and in the deer, the so named ligament received numerous nerves, that its texture bore no resemblance whatever to ligament, that it became rudimentary in those animals whose sight was feeble, which would not necessarily happen were it simply a ligament for the suspension of the tunics and humours of the eye; the conclusion was irresistible; that the annulus albus is a muscle, that it is the muscle by which the eye adapts itself to the perception of distant objects, and that by it, in conjunction with the iris, all the changes which take place in the interior of the eye-ball are effected *.

In compliance with the wish of some of my friends, small sections of the annulus albus were submitted to the microscope. The result of this investigation was, that it had no resemblance to a ligament; that it contained comparatively large branches of nerves; that it did not resemble any of the textures of the eye-ball except the iris, but that here the resemblance was so close that they could with difficulty be distinguished.

It were desirable that I should bring forward ample collateral proofs of the presence of this great central muscle of the eye-ball, and which I shall henceforward call the Ciliary Muscle; but I am by no means prepared for so extended an inquiry: many, however, will readily suggest themselves to those who have investigated the eye anatomically in a variety of animals. Birds, for example, that have a strong ciliary muscle, amply supplied with nerves, have powerful vision; their perception of ob-

* The opinion that the annulus albus is muscular, has been often maintained, but, so far as I know, no proofs, derived from anatomy, of its muscularity, have ever been laid before the public.

jects placed at a vast distance is tolerably distinct. It is strong in man, and in the quadrumana; weak in the ox and sheep, but still weaker in the horse; very powerful in the deer tribe, and no doubt in the antelope. I had often remarked, without being able to assign a reason, how greatly the antelope and deer trusted to the organ of vision, and how inferior their organs of smell were to those of the horse and ox. Whoever will carefully attend to the habits of these animals when allowed to roam about at large, may observe, that the horse and ox fearlessly enter thickets, trusting to the acuteness of their smell for the discovery of wild ferocious animals, whilst the antelope prefers the open plains, and seldom resorts to the bushy country, unless urged to it by a want of herbage and water *. Horned cattle, with most ruminating animals, graze generally towards the quarter whence the wind blows; and, though I am aware that other

* I observed a very singular fact in Africa, which first awakened my suspicions relative to the defective vision of the horse. In that country we were forced, from a deficiency of pasturage, to allow our horses to graze at perfect liberty on the open desarts, and they, so situated, seemed to acquire many of the habits which the animal would probably possess in a perfectly wild state. They grazed generally in small troops, to which an entire horse, or one of the boldest of the geldings, seemed to serve as protector; on the approach of strangers, the troop immediately collected into a circle, and remained so until the horse appointed to watch over the general safety had ascertained whether or not danger was to be apprehended, by a nearer approach of the object suspected. On one occasion, having gone into the fields with a few friends, of whom one was dressed in a morning gown, and, coming unexpectedly on the troop of horses, they were observed to collect immediately into a circle, and to detach one of their number, with a view to ascertain the nature of the very unusual appearance, which they evidently saw but indistinctly, though scarcely three quarters of a mile removed from the place we stood. It was now I remarked, with some surprise, that the horse did not, during the very long and circuitous course, approach much nearer to us, but made hastily for that situation in which we should be placed between him and the quarter from whence the wind blew; thus evidently employing the organ of smell in preference to that of sight.

reasons have been assigned for this, yet it evidently serves to give them timely warning of the approach of their enemies. In fishes, which have neither ciliary muscle nor nerves, nor true moveable iris, the powers of vision are extremely limited. It may also be readily imagined why, in those persons from whose eyes the lens has been extracted in the operation for cataract, the customary powers of the eye should remain nearly as strong as previous to the operation, and that, on the adaptation of a compensating lens for that which has been removed, the person should feel no difficulty in adjusting the eye as before.

But I do not suppose that the whole of the phenomena of the adjustment of the eye to the perception of objects placed at various distances, depend entirely on the ciliary muscle. I have every reason to believe, that the perception of objects placed at very short distances, depends altogether, or nearly so, on the contractions of the pupil or iris. I am quite aware of several objections which might be offered to this theory, to obviate some of which, I shall remark, that the eye, when not particularly employed, remains in a middle state, *i. e.* we of preference regard objects situated at moderate distances from us; or, it may be said, that we continually alter the focus from distant to near objects, and *vice versa.* Now, when these changes go on rapidly, there is little or no sense of fatigue, just as happens when the various muscles of the body are put in action alternately, so that gentle walking is always less fatiguing than standing still in precisely the same position. If an individual be directed to regard a distant object, the most obvious change perceivable to the bystander is a sudden dilatation of the pupil, which dilatation is necessarily in the ratio of the distance of the object, and the colour of the iris. If the person be requested to examine the same object with very great attention, the pupil will be seen gently and slowly to contract, no doubt to throw out, as much

as possible, all the unnecessary rays of light; this exertion may be continued for some time, but every one knows, that it at last becomes so irksome, that the eye involuntarily returns to a middle state. We might suppose this sense of fatigue to arise from our attempts to contract the iris, but this is by no means probable; it seems, to me, to arise from the exertions of the ciliary muscle, for it is extremely difficult to imagine why fatigue should arise from a moderate contraction of the iris in viewing distant objects, when we know that the same membrane is generally much more contracted in its ordinary state. Moreover, the eye cannot long regard distant objects without fatigue, even though the attention be not particularly fixed thereon; now it cannot be the iris in this case. Lastly, Distant objects are seen with tolerable precision when the pupil has been dilated by the action of belladona; now, here, the iris can scarcely be supposed capable of much action. If a person who has been for some time regarding distant objects, be now requested to examine one very near, the pupil instantly contracts, and continues to do so in proportion as the object is made to approach the eye. At last the pupil reaches its maximum of contraction, and if the object still approach the eye, the fatigue of distinct perception becomes insupportable; the iris suddenly returns to a middle state of contraction, and the object is no longer perceived. When belladona has been applied to the eye, near objects cannot be perceived, and the eye is said to have changed its focus, and to have become long-sighted. These expressions are incorrect, the eye has lost its power of viewing very near objects, because the iris can no longer contract. It is also stated, that distant objects are perceived as well after as before the use of the belladona, of the accuracy of which assertion we may reasonably entertain doubts. The inner membrane of the choroid has been well investigated by anatomists; there can be little doubt of its forming the cili-

ary body and uvea *. It appears, at first sight, a very mechanical notion, that the ciliary folds and processes are in a great measure formed, by a membrane being made to occupy a circle naturally too small for it in its expanded state; but the way in which these folds commence on the inner surface of the choroid, actually bears out this idea, for the membrane may be unfolded to a very great extent, and thus made to cover a much larger surface. The same remark is applicable to the internal ciliary folds or processes †. The external, or true ciliary processes, have been admirably described and pourtrayed by Zinn, and are, indeed, too well known to all anatomists, to require any particular notice here. Near their base, and between their intervals, they are firmly fixed to the capsule, covering the canal of Petit by a mechanism which I shall now endeavour to describe. It has always been asserted, that when the ciliary processes are removed from the humours, their impressions only remain; but this is incorrect. I at first thought that a portion of their substance was left adhering to the capsule over the canal of Petit; but further, and a more careful examination, proved this opinion to be erroneous. When the iris is drawn backwards, after having removed the cornea, and allowed the aqueous humour to escape, the anterior edge of the ciliary processes may be seen projecting into the posterior chamber of the aqueous humour; and on drawing these also backwards, a number of parallel fibres are seen proceeding from the marginal or equatorial edge of the capsule of the lens, to be inserted between each of the ciliary processes. These fibres lie immediately over the canal of Petit, and contribute with the perfectly transparent membrane lying

* Some anatomists have confined their description of the uvea to the coating of the pigmentum nigrum, found on the posterior surface of the iris; but this is not generally received.

† These are described a little below.

immediately beneath them, to form a portion of the parietes of this canal. If we now examine the eye in the opposite direction, *i. e.* by dissecting off the sclerotic and choroid tunics, and the retina, we may perceive a similar range of fibres proceeding from the anterior edge of the vitreous humour, and from the point where the retina terminates, forwards and upwards, to be in like manner connected with the ciliary processes, by passing in between each. If a lateral view be taken *, by making a very delicate section of the eye, and gently raising the cut edges of the ciliary body, there is still the same appearance, viz. of anterior and posterior fibres, which have a common insertion between the ciliary processes near their base, and which form, in conjunction with a transparent membrane, the external paries of the canal of Petit. When we attempt to remove the ciliary processes, and to detach them from the lens, some force is required, and it not unfrequently happens, that the processes themselves are torn, and a vast quantity of the pigmentum nigrum effused; at other times they may be detached, leaving the whole of the semitransparent fibres lying over the outer paries of the canal of Petit, and the canal itself perfectly untouched. The same result may be obtained by maceration; but as it was evident that, in separating the transparent humours from the dark tunic of the choroid, a connection had been destroyed essential towards understanding the anatomy of one of the most important parts of the eye-ball, I re-examined the whole with great attention, in numerous specimens, macerated for a long time in spirits.

In order fully to describe the anatomy of these parts, we ought to commence with the retina, or, perhaps, with the membrane sometimes found between the retina and choroid coat. This membrane appears to have been known, and partly described, by the late Dr Monro; its existence has been often denied and re-asserted; on referring to my Notes, taken during

* See Plate III. Fig. 2.

the dissections, I find it stated, that the membrane is sometimes partially absent, but, perhaps, never entirely; that it seems to be destined for the conveyance of bloodvessels, and, possibly, lymphatics *, and is apparently connected with the secretion of the pigmentum nigrum; for, in those animals in which the pigment is very fluid and abundant, the membrane seemed most distinct; and, on the contrary, was altogether deficient, in those in which the pigment seemed to form a part of the choroid itself. It is difficult to say precisely where the membrane terminates; but there is little doubt of its being expanded over the membranes forming the canal of Petit, and that it may even extend to the edge of the pupil.

We may now proceed to examine the retina, because the anatomy of its termination is intimately connected with a very beautiful and very singular structure, not yet sufficiently described. On its external aspect the retina is apparently inclosed by a membrane, adhering to it so closely, that it cannot be demonstrated apart. From some successful dissections, I am inclined to consider the numerous bloodvessels seen on the internal surface of the retina, as expanded on an excessively delicate tunic. These fix down the terminating edge of the retina all round the anterior edge of the vitreous humour, and adhere firmly to the capsules forming the external paries of the canal of Petit. Whether or not the external parietes of the canal of Petit be a mere continuation of these membranes, conjoined with the hyaloid, is a matter of little importance; it is sufficient for our purpose, that they seem continuous, and are firmly connected to each other. In whatever way formed, the membrane, or assemblage of membranes, proceeds forwards, to be inserted into

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* DR PORTAL, in one case, found hydatids situate between the choroid and retina. Unfortunately, he does not describe the pathology of the case with sufficient minuteness; the fact, however, is valuable.

the circumference of the capsule of the lens, forming in its passage numerous longitudinal folds, and small projecting fimbriated bodies, by which, in a natural state, the transparent humours are connected with the superjacent ciliary body. When examined with a good glass, these folds are remarkably distinct, and the whole bears the closest resemblance, in its distribution, to the true ciliary body and processes. I have, therefore, ventured to call them the Internal or Transparent Ciliary Body, (or the ciliary body of the hyaloid membrane), in contradistinction to that of the choroid.

From the internal surface of the transparent ciliary body just described, is detached a membrane, which being inserted into the capsule of the lens, somewhat more posteriorly or central, thus contributes to complete the triangular shaped canal of Petit *. A number of appearances will immediately suggest themselves, which the above details readily explain; such, for example, as the continuity of all the parietes of the canal of Petit, after the removal of the transparent humours from the eyeball, and the irregularities which the external paries of the canal presents, when distended with air, which induced the French to call it "godronnée," and is owing to the bulging out of the delicate tunic occupying the intermediate spaces of the folds or processes, which being strengthened, and more firmly bound down, do not expand like the other parts of the membrane.

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* This may be considered as the hyaloid itself. Had I been aware at the time these dissections were performed, that an excellent continental anatomist (M. Ribes), had formed similar opinions regarding these internal ciliary processes, I should, perhaps, not have repeated my dissections so often as I did; but I was forcibly struck with the evident incorrectness of the descriptions given in some of the latest and best elementary works on the Anatomy of Man; and I was, moreover, anxious to disprove the idea lately adopted, that these folds or processes are fibrous or muscular bodies. M. Ribes' work has not yet reached this country; and I allude simply to a brief notice contained in an early number of the "*Bulletin de Sc. Med.*"

By means of these transparent membranes, which I have called the Internal Ciliary Processes, (or the ciliary processes of the hyaloid membrane), the vitreous humour and lens are intimately united together; but it is also, as I have already shewn, by means of the same membrane and processes that the humours of the eye are affixed to the tunics immediately connected with them, viz. the external ciliary processes, ciliary muscle, sclerotic, &c.; for the internal ciliary processes pass in between the folds of the great or external ones, throughout a great part of their course, and anteriorly near their termination in the capsule of the lens, they send up numerous processes, to be inserted into the superincumbent ciliary body between each of the ciliary processes. Some have supposed these reduplications of the *membrane* to be muscular fibres, an opinion against which we have both ocular inspection and analogy. I am not prepared to assert that microscopic fibres do not exist, but these, from their very nature, must be exceedingly unimportant.

Of the Pigmentum Nigrum and its Membrane.

The late Dr Monro observed, in two cases, a membrane shutting up the pupil, the result of inflammation; and in both the posterior surface of the membrane was coated with pigmentum nigrum. From this, and some other observations, I am inclined to think, that the pigmentum nigrum, in many animals, is inclosed in a peculiar membrane, and that this membrane may be continuous with that lying immediately external to the retina, and betwixt it and the choroid coat, and generally known by the name of the Membrane of Jacob. It is true, that throughout a great part of its course, this membrane, when present, is semitransparent, and that which sometimes incloses the pigmentum nigrum is dark and opaque; but I have already demon-

trated, that this happens with the reflected membranes of the choroid, and with several other membranous tissues of the eye. In this case we should consider the membrana Jacobi as extending to the very edge of the pupil. The appearances, in the eye of the cassowary, and in fishes, prove that there does exist a membrane in the situation described.

The anatomy of the fishes' eye, which I shall now very briefly describe, supports these opinions. Though a few striæ may be observed on the inner surface of the choroid in fishes, yet they cannot be considered as constituting a ciliary body, whilst it is very evident that there are no ciliary processes. The attachment between the choroid and humours is by no means firm, but still it exists, and the mode is as follows: On tearing off the sclerotic and cornea, a greyish-white line or chord, of considerable thickness, is left. The choroid evidently passes under it, to invest the internal surface of the transparent iris *; but they cannot be easily separated from each other. The proper choroid, and innermost layer of this compound membrane, which had been separated from each other posteriorly by the choroid gland, unite intimately where the retina terminates, to pass forward beneath the iris. A dark coloured fluid pigment covers the whole surface of the retina, separated from it, however, by a thin membrane; but from the termination of the retina forwards, to the very edge of the pupil, this pigment assumes the form of a striated close membrane, perfectly black, and much resembling the ciliary folds in the mammalia, and even possessing, as in the cod, a beautiful fringed border, by which it adheres to the capsule of the vitreous humour. In other words, the pigmentum nigrum is no longer deposited on the surface of the choroid, but

* *i. e.* Supposing an iris to exist betwixt the prolongation of the choroid and the reflected membrane of the cornea, which I do not believe to be the case.

becomes incorporated with a membrane. This takes place immediately anterior to the termination of the retina. Though the iris, and all the choroid coats, be removed, it is evident, that there is still a very thin transparent membrane betwixt you and the retina, connecting this latter with the membranous pigmentum nigrum, and extending forwards to the edge of the pupil. When the retina is pulled towards the optic nerve, this membrane is drawn along with it; when detached, the dark coloured membrane assumes a fimbriated edge. The termination of the retina is very precise and determinate; the dark coloured membrane adheres firmly to the subjacent hyaloid, but may be reallily separated, for a certain distance, by commencing at the inner edge of the pupil; but on approaching the terminating edge of the retina, the union becomes very intimate, and a junction of all these membranes is very evident, by means of a circular firm line, visible even to the naked eye. I have not observed any adhesion of this dark coloured membrane to the crystalline, and am, indeed, certain of the contrary. At its *anterior* edge, it quits the hyaloid, and proceeds to form that dark membrane lining the iris, whilst the hyaloid, clear and transparent, advances forwards to inclose the lens.

The ease with which the retina may be separated from the black membrane, shews that it lies external to the process of the membrane connected with the hyaloid; and this becomes evident, by observing the retina preserve, on being detached, its anterior formal outline, or termination, and by the black membrane still adhering to the hyaloid, which appears quite smooth and continuous; the anterior part of the retina is then, as it were, inclosed between two folds of the membrane of the pigmentum nigrum, the exterior of which, no doubt, was described long ago; vessels unite it with the internal part of the iris, to which, at these points, it becomes firmly attached.

Perhaps the most remarkable appendage of the choroid is the marsupium, supposed to exist only in the eyes of birds, but which may be demonstrated in a great many fishes, and, I may add, reptiles. The marsupium is a membranous expansion, sufficiently firm, extending from the point at which the optic nerve expands, into the retina, and advancing generally through the centre of the vitreous humour, to be fixed into the posterior and lateral surface of the capsule of the lens, or, more correctly, into that portion of the hyaloid membrane covering the posterior surface of the lens. The marsupium is coated inside with a kind of pigmentum nigrum, in some birds for about two-thirds of its course; in others much more; the remaining portion, or that by which it is attached to the choroid, is in many birds quite transparent, and thus has escaped the notice of the anatomist. This portion is, moreover, exceedingly delicate, and, unless the eye-ball be opened with the greatest caution, is seldom seen. In the eye of the cassowary it is of a brownish colour. In proportion as the true anatomy of the marsupium was unknown, so were its functions deemed mysterious and important; its presence was supposed confined to the feathered creation, and it thus became associated with the superior vision of birds. But anatomy demonstrates, that it is simply a continuation of the choroid, that portion being generally transparent by which they are connected *.

I have already observed, that the portion of the choroid which passes over the termination of the optic nerve, is transparent, whilst, in those animals in which there is no considerable artery passing through the vitreous humour, but where the vessels proceed to the lens immediately under the retina, their vessels will be found to be supported by a delicate transparent

* Plate III. Fig. 4.

membrane, which may be considered as an expanded transparent marsupium *. Viewing the anatomy of these parts in the *comparative range* of animals, we observe, that, in the cat's eye, the retina passes onwards to its destination unaided by any very distinct membrane; the vitreous humour is eminently transparent, and the circular point by which the optic nerve penetrates the sclerotic is translucent. If the choroid transmit any membrane across the termination of the optic nerve, it must be excessively thin, and, to common glasses, altogether inappreciable. But, already in the horse, the passage of the choroid over the optic nerve, and the escape of the medullary substance, to form or be connected with the retina by innumerable small foramina, is visible to the naked eye. In the eye of some fishes, as the cod, the principle is the same, though somewhat varied; the retina is divided into two portions, betwixt which a prolongation of the choroid passes, to form a distinct, though partly colourless marsupium. Lastly, In birds, we find a true coloured marsupium †.

I have found it more difficult to decide on the functions of the marsupium than on its true anatomy; but if we cannot shew what it is, we can at least demonstrate what it is not. As it is in no way muscular, it cannot serve to alter the relation of the internal parts of the eye to each other, but it may assist in retaining the lens in its situation, and give support to the numerous vessels proceeding to it. It has no nerves.

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* May not this membrane be the same with the dark coloured tunic, formed between the retina and vitreous humour, in the larger varieties of the *cephalopodous* mollusca?

† It has appeared to me, that, in many animals, and even in some of the ruminantia, the central artery of the retina does not send any branch through the centre of the vitreous humour.

I ought now to speak of the mode in which the optic nerve enters the eye-ball, which could not have been done previous to describing the anatomy of the choroid and marsupium. It will be unnecessary to detail the more usual facts, as they must be already well known to those whom I have the honour of addressing. I shall merely observe, that, in the mammalia generally, the optic nerve passes through the choroid by innumerable little foramina, or, perhaps, by short canals, to expand into the pulpy membrane of the retina, always supposing that the retina is an expansion of the optic nerve *. The change relative to the entrance of the optic nerve commences in the ruminantia, for we may observe, that, even in the sheep, the point at which the optic nerve expands into the retina is not circular, but somewhat semicircular. In the eye of the American deer †, the circular mode of entrance has entirely disappeared, and the nerve presents, on entering the eye-ball, a small segment of a very large arch. In the eye of the fallow deer, the line by which the nerve enters the eye is nearly straight, and greatly lengthened. Lastly, In that of the bird, it is completely so. In the latter class of animals, the nerve escapes on all sides through the reflected choroid; in others, it merely passes through the choroid, stretched over the entrance of the nerve, when such happens to be the case ‡. I have, throughout this paper, alluded often to the anatomy of the eye of the deer; it approaches very nearly that of

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* Some anatomists think that the optic nerve is merely distributed on the retina. There are several analogies in favour of this opinion, and even direct ocular inspection in the eyes of some fishes.

† The animal was said to have been sent to this country by his Excellency the Governor-General of Canada.

‡ It is probable, that, in all animals, the choroid passes directly over the entrance of the optic nerve; but it would be extremely difficult to demonstrate this, owing to the excessive tenuity and transparency of the membranous expansion.

the bird, and forms, as it were, the intermediate link, by which the classes Mammalia and Aves are, so far as regards vision, united together. I have already explained, why, in the deer, though the mode by which the optic nerve enters the eye be the same as in birds, there is still no distinct marsupium formed, because the chief vessels do not pass through the vitreous humour, but accompany the retina. As the optic nerve passes through the sclerotic in birds, this latter membrane forms, as it were, a lengthened sheath (by a separation of its tunics) in which the nerve proceeds for some way, before it penetrates into the interior of the eye. In consequence of this peculiar form of the sclerotic, the termination of the nerve which regards the vitreous humour, is bounded by two strong, tendinous, straight lines; these are, of course, formed by the edges of the sclerotic. The same distribution of the sclerotic and nerve takes place in the deer, and extends even to the passage of the vessels, from the exterior to the interior parts of the eye, mid-way between the optic nerve and cornea. At these points (four or five in number), the choroid, by the transmission of prolongations, evidently communicates with the exterior of the eye-ball *. The vessels do not pass directly through the sclerotic, but between its layers; one edge of the sclerotic overhangs the other, and the more projecting line is strong, tendinous, and fixed down to the inner surface of the sclerotic. But it is not in these points only that the strong resemblance of the deer's eye to that of the bird holds good; the great ciliary muscle is remarkably broad and distinct,

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* In the number of the *Philosophical Transactions* for 1810, (I quote from memory), four distinct muscles are described within the sclerotic, in the eye of the rhinoceros. The situation of these muscles corresponds exactly with the prolongation of the choroid membrane described in the text. I have examined the eye of the African or two-horned rhinoceros, but do not remember to have observed any such muscles in it.

and the ciliary nerves are numerous, and send branches round the iris, in the same way as in birds *.

In most fishes there is no true or moveable iris. In the mammalia its relations appear complex; but they may be unravelled, by comparing the structure with birds. I shall here describe very briefly its anatomy in the latter animals, and the mode in which it may be best displayed. If we select the eye of the larger birds, we may, unaided by glasses, perceive that the pigmentum nigrum, covering the uvea, is inclosed in a delicate and distinct membrane, and that this membrane proceeds from the anterior part of the ciliary processes. The expansion of the inner membrane of the choroid into the ciliary bodies, and then into the uvea, is also most distinctly observed; the formal termination of the middle layer, or true iris, in a brownish cellular membrane, shewing clearly that it is a peculiar body, and not a continuation of any other membrane; and the connection this cellular membrane has with the cornea, and with the tendinous expansion of the ciliary muscle.

* I ought to have inserted, in this part of the observations, the result of my inquiries into the retina, its distribution, nature, &c.; but at the time this memoir was presented, I did not deem the researches sufficiently complete, or fitting to be submitted to the Royal Society of Edinburgh. But having repeated some of the dissections, and (the opportunity having presented itself) extended the researches, I made the important, and most unexpected discovery, that the *foramen centrale* of the retina, generally called the *Foramen* of SŒMMERING, is not confined to the eye of Man, and of some Quadrumanous Animals, whose organisation somewhat approaches him, but is extended to the class *Reptiles*, contrary to the opinion of all comparative anatomists. The details of this very singular discovery are reserved for the Memoirs of the Wernerian Society, and the fact is simply announced in this place, to make my other observations more complete. " The *foramen centrale* of the retina exists in many lizards, as the *superciliosa*, *scutata*, *striata*, and *Calotes*, and is in them comparatively much more developed than in Man; but it is wanting in the lizards called *gecko*, *mabuya*," &c. See *Memoirs of the Wernerian Society*, Vol. v. Part 1.

There is still another mode of displaying the structure of the eye in birds, equally advantageous with that described. Make an incision through the ciliary body, immediately anterior to the ciliary muscle, and a cavity will be opened into, lying immediately over the ciliary plexus. This cavity is bounded internally by the ciliary body; externally by the ciliary muscle and nerves, and their investing membrane; posteriorly, by the connection of the ciliary muscle with the choroid; anteriorly by the membranous expansion connecting the iris, ciliary body, cornea, and anterior termination of the ciliary muscle, to each other. This cavity may, for short distances, be filled with air by the blowpipe. It has never been described, though it exists in the eyes of the mammalia. Where the ciliary nerves assume the ribbon form, or become a plexus, the canal is interrupted by the passage of the nerve across it, and by cellular membrane; but it is to be observed, that, in most of the mammalia, in which there is no true plexus of ciliary nerves, the canal is small, and partly filled with a very rare cellular tissue. The anatomy of this cavity displays the whole anatomy of the anterior part of the eye-ball; it shews that the anatomist has confounded the ciliary nerves with the ciliary muscle, and it demonstrates the influence which the motions of the ciliary muscle and iris must have over the internal configuration of the eye-ball. Numerous branches of nerves proceed to the ciliary muscle and iris, corresponding to the exertions they are required to make in this class of animals.

The anatomical distribution of these parts differs but little in the mammalia from that just described, and the analogy may be deemed correct. The triangular cavity immediately anterior to the insertion of the ciliary muscle into the sclerotic, is not so capacious, and the muscle is but indirectly connected with the internal layer of the cornea. It may be readily imagined, that certain physiological differences will arise out of the anatomical deviations just described. It has been often as-

serted, that the base of the iris is fixed immediately into the ciliary muscle, but this is incorrect; for, though it seems to be so in some mammalia, yet in birds, and even in most of the mammalia, we find a considerable space intervening betwixt the base of the iris and the anterior edge of the ciliary muscle, which space is filled up by a dark coloured loose cellular tissue.

In the commencement of this Memoir, I have made a few remarks on the Functions of the Iris, and, that I may not seem to have overlooked the subject, I may observe, that many * have ascribed muscular fibres to the iris, and, recently, Dr Maunoir has endeavoured to shew two sets of fibres, the one external, radiated, the dilator of the pupil; the other internal, narrower, composed of circular fibres, the constrictor or sphincter of the pupil. I have examined the iris very carefully in a great variety of animals, but have never been able to detect any muscular fibres as forming a part of its structure; yet it is evidently of a peculiar nature, and possesses in a high degree the power of motion. It is most abundantly supplied with nerves and bloodvessels, and, like other muscles, contracts under the influence of galvanism †.

* I have not been able to procure a copy of M. Maunoir's little treatise on the subject, so that I quote the passage from the "Traité D'Anatomie Descriptive," by Dr Cloquet. Neither have I been able to obtain a sight of some observations which I find announced by the Journals, as having been made on the same subject by Dr Edwards of Paris. I regret this the more, because, from the known accuracy of that excellent physiologist, I feel well assured, that, on the points investigated by him, he would leave little to be done by future observers.

† Mr John Hunter, whose physiological opinions scarcely admit of question, seemed to think, a short time previous to his lamented death, that a fibrous structure was not the *sine qua non* of muscularity. The application of this to the textures of the iris and ciliary muscle is obvious.

We shall discuss, whilst considering the ciliary nerves, the long agitated question, Whether the iris be a voluntary or involuntary muscle. On the death of an animal, the pupil is in general much dilated, but it again contracts whilst the other muscles of the body are becoming rigid, these phenomena being analogous. I have fancied that, in the horse, some time after death, the pupil is more contracted than in most animals, which may be attributed to the superiority of the sphincter over the dilating part of the iris, and this is probably connected with the peculiar fringe in which the superior margin of the pupil terminates, compared not unaptly by Swammerdam to the fringed curtain-like body in which a part of the pupillary margin in the eye of the skate terminates. There is a similar appearance, though not fringed, nor yet so distinct, in the eye of the common dolphin.

III. *Of the Ciliary Nerves, their Nature, and Distribution.*

I regret that it will not be in my power to enter into any very lengthened details relative to the ciliary nerves, and that I shall be necessitated (by other avocations) to limit myself to a mere outline, drawn up hastily, and even carelessly, though from numerous dissections.

The nerves supplying the iris and ciliary muscle are derived from the third and fifth pairs of cerebral nerves. They generally form a ganglion, previous to giving off the true ciliary nerves; which ganglion is sometimes wanting, and would seem to have but little influence over the action of the nerves. It resembles anatomically, and perhaps physiologically, the ganglion formed

by the superior and inferior maxillary branches of the fifth, and is probably intended to subdivide the nervous filaments, and afford a nucleus for the origin of a greater number of nerves than could otherwise have happened, had they arisen from a single trunk. That this is nearly the sole use of these ganglia, we may infer, by observing that they are often wanting in animals, without giving rise to any difference in the functions of the nerves. There is no lenticular ganglion in the horse, and this explains the scarcity of ciliary nerves in that animal, the little development of the ciliary muscle, and his very limited powers of vision. The submaxillary ganglion, and that so well described by MECKEL, are wanting in a great number of animals; and the latter is not unfrequently absent in man himself. There is nothing, therefore, important in these ganglia; nothing affecting directly the functions of the parts to which the ciliary nerves are distributed. The connexion of the great sympathetic nerve with the lenticular ganglion is very indirect, for the supposed connecting branch cannot be traced to the ganglion, and seems rather to join the ophthalmic branch of the fifth, in the same way as the superior maxillary is connected with the superior cervical ganglion, by means of the vidian.

The iris has been long considered an involuntary muscle, and undoubtedly it is not so immediately under the power of volition as many other muscles, and more particularly those of the extremities; but the assertion that the iris is altogether an involuntary muscle, and dependent for its motions on the stimulus of light acting on the retina, may, I think, be disproved by the simplest experiments. The iris dilates or contracts according as the attention is more or less energetically fixed on an object, though the quantity of light transmitted to the retina remain precisely the same. The cat and owl have the most complete command over the motions of their iris,—a fact well known to every one;

and such is the power possessed by the parrot over this membrane, that some have supposed the iris to be in this bird a voluntary muscle. But there is no necessity for resorting to such a supposition, for it is nearly as voluntary in man as in the parrot *.

The ciliary nerves are remarkable for their size and number in birds, in the quadrumana, and in man; they are very considerable in the deer, remarkably distinct, readily traced into the ciliary muscle, and send branches round the iris at about an equal distance between the margin of the pupil and base. This distribution is very remarkable in the hawk and owl, and may be easily seen without dissection. A similar distribution prevails in all the birds I have dissected, and even in many quadrupeds; in the former, the ciliary nerves pass through the ciliary muscle in large trunks, and having reached the space or cavity existing between the ciliary muscle and base of the iris, there form, or are connected with, an extensive plexus of nerves, which passes all round the eye-ball, and has been mistaken for the ciliary nerves themselves. From this plexus arise a vast number of very delicate nerves, which are distributed chiefly to the ciliary muscle, and form partly on its inner surface, and partly within its substance, a complete net-work of nerves. The meshes of this net are large, and, at the junction of some of the nervous branches composing it, delicate ganglia or swellings are perceivable. The iris seemed to me to receive but a few nerves from the great plexus, and that by far the greater proportion of those almost innumerable fibres which are distributed to it, proceeded from a few large trunks of nerves which pass immediate-

* Were we to suppose, that, in the cat, owl, and parrot, the movements of the iris are dependent on volition, it would prove an unsurmountable objection to the doctrines which teach that the nerves of volition never pass through ganglia.

ly to the base of the iris, and are not so intimately connected with the plexus itself, except by communicating branches. Where very distinct, as in birds and in deer, they send large branches round the iris, imbedded in its substance, and from these arise numerous nervous fibrils, supplying every part of the iris. In those mammiferous animals in which the dissection can be clearly made out, either from the magnitude of the nerves, and the structure of the parts to which these are distributed, as in the deer, or from structure only, as in the horse, we find, that the ciliary nerves may be divided into two distinct sets, viz. one passing to the ciliary muscle, and the other to the iris. The former seem the least numerous, and are in some animals very small; the latter, with a few exceptions, are tolerably abundant in all the mammalia.

In the horse, the nerves of the iris do not pass through the ciliary muscle, but under it, *i. e.* between it and the choroid coat, whilst the nerves supplying the ciliary muscle pass in long chords along the inner surface of the ciliary muscle itself, constantly diminishing in size, by reason of the numerous fibrils sent to the muscle. We thus discover the cause of the error lately committed by those very excellent anatomists, who fancied the ciliary muscle to be a ganglion, or at least a nervous plexus; for they saw the ciliary nerves suddenly entering the muscle on their way to the iris, and as it were disappearing; they perceived, moreover, that other branches seemed to arise from the ciliary muscle, and they hastily concluded, that the intervening substance must be a ganglion or nervous plexus at the least; but I have shewn this to be a great error; for the smaller branches of those nerves which enter the ciliary muscle are distributed to it, whilst the larger ones pass directly through, to be expanded in numerous fibrils on the true iris. We may now readily explain the mistake of those who fancied they discovered nerves proceeding to

the cornea. They found nerves going towards the ciliary muscle, which were lost at the point where the muscle is inserted into the sclerotic: they imagined, therefore, that these nerves must be distributed to the cornea, as it was difficult to conceive how any should be sent to the sclerotic.

I am fully convinced, from numerous dissections, that the so named ciliary ganglion in man, and in the mammalia, is a pure fiction, and imagined with a view to support the hypothesis of the iris being altogether an involuntary muscle. As it is my intention to resume the subject shortly, I shall here merely observe, that nervous plexuses, as those with which the nerves of the extremities are connected in the mammalia, and the iridian nerves in birds, are placed there by nature, not to impede the influence of the brain, exercised during volition, over the muscles to which these nerves are respectively distributed, but to afford a more extensive point of connection to a much greater number of nervous trunks than could have been the case, had the branches connecting the plexus with the brain and spinal marrow, proceeded uninterruptedly to their termination in the respective organs. The same observation is applicable, slightly modified, to the ganglia found in the course of the cerebral nerves, and in those of the vertebral column; it is to the sympathetic system of nerves, and their ganglia only, that the discovery of our celebrated countryman JOHNSON is applicable. It must be evident to the Society, that to enter further into the discussion of these doctrines, would lead me quite from the subject of the Memoir.

The nerve proceeding towards the choroid gland in fishes, is a branch of the third, and not of the fifth; moreover, it cannot be traced to the gland. The remaining branches of the third are distributed to the external muscles of the eye-ball. The sixth pair of cerebral nerves in fishes join the fifth within the cranium.

There are no ciliary nerves, and consequently no true iris nor ciliary muscle, in the specimens I have dissected; but, as the species I have had an opportunity of examining have been but few, it is not intended that these observations should apply to the whole class of fishes.

EXPLANATION OF PLATE III.

Fig. 1.

Distribution of the ciliary nerves to the ciliary muscle and iris, in the eye of the Cassowary.

The choroid tunic has been cut through, where it lies over the plexus of nerves, and the cavity described at page 28 of the Memoir.

a The posterior surface of the Iris, generally called *Uvea*. On it may be seen the remains of the membrane of the Pigmentum nigrum.

b The Ciliary Muscle.

c The posterior surface of the Cornea.

d The cut edges of the Sclerotic and Choroid.

e The Ciliary Processes and Striæ.

f The external surface of the Ciliary Body removed from its situation to shew the cavity containing the Ciliary Nerves.

Fig. 2.

The Anatomy of the internal Ciliary Processes, as seen in the Eye of the common Ox.

a The Iris cut up and reflected; the cornea has been removed.

b The Lens *in situ*, inclosed in its capsule.

c The internal Ciliary Processes seen anteriorly.

d The same seen posteriorly; the choroid has been removed. These different portions of the internal ciliary processes are quite continuous, but are firmly attached to the external ciliary processes immediately beneath the point *f*. At *c* the external ciliary processes have been entirely removed, and an opening made into the Canal of Petit.

Fig. 3.

A section of the Eye of the Heron.

a The internal Ciliary Processes surrounding the margin of the Lens.
a a The Lens covered by its capsule.
b A portion of the Vitreous Humour and its capsule.
c The Marsupium; *p*, the very delicate and sometimes transparent portion by which it is united to the choroid.
f The Choroid.
e The Sclerotic.
i The Ciliary Plicæ, (their internal surface).
g One of the sheaths, transmitted by the choroid through the sclerotic, inclosing blood-vessels. These sheaths are very remarkable and distinct in the eye of the fallow-deer, American deer, &c.

Fig. 4.

A section of the Eye-ball of the Cassowary, to shew the mode in which the Marsupium is formed.

a The Marsupium. The lens has been removed.
b A portion of the Marsupium, (where this membrane meets the choroid, and is continuous with it), removed, to shew the strong white medullary line of the Optic Nerve.
c The Choroid.
d The Ciliary Body and Plicæ.
e The posterior surface of the Iris, covered by the membrane of the Pigmentum nigrum.
f The Sclerotic.

III. *Notice of an undescribed Vitrified Fort, in the Burnt Isles, in the Kyles of Bute.* By James Smith, Esq. of Jordanhill, F. R. S. Edin.

(Read March 17. 1823.*)*

In the month of September last (1822), when becalmed in my cutter in the Kyles of Bute, I accidently landed on the most northerly of the Burnt Isles, a small group that stretches across the Kyle or narrow channel between Bute and Argyleshire.

From the appearance of a ridge nearly covered with turf, I imagined at first that kelp had been formerly burnt here, but on examining it more narrowly I discovered that it was caused by the remains of a vitrified fort.

The island on which it is placed is a flat gneiss rock, with about half an acre of vegetable soil on its summit. The fort is placed at the southern and most elevated extremity, but is not more than twelve or fifteen feet above high-water mark. The walls form a circle, or rather an irregular polygon, about sixty-five feet in diameter, occupying nearly the whole of the highest end of the island. I could trace the vitrified matter all round, and should imagine, from what remains of the walls, that they were originally about five feet in thickness. They seem to be entirely composed of the gneiss which forms the rock of this and the surrounding islands. Many of the stones have decayed by the action of the atmosphere, previous to vitrification, and most of them have been acted upon by the intense heat of the fire, although in very different degrees. Some of them are but slightly glazed, whilst in others the felspar appears to be converted into a dark

brown glass, either run into considerable masses, or into veins alternating with the strata of quartz, which has become granular like freestone: occasionally the vitrified matter forms a white enamel.

I know not whether any more easily fusible substance has been used as a flux, but I could not observe any appearance of breccia, which Dr MacCulloch, in his paper on Vitrified Forts, in the second volume of the Transactions of the Geological Society, states to have been generally used for that purpose.

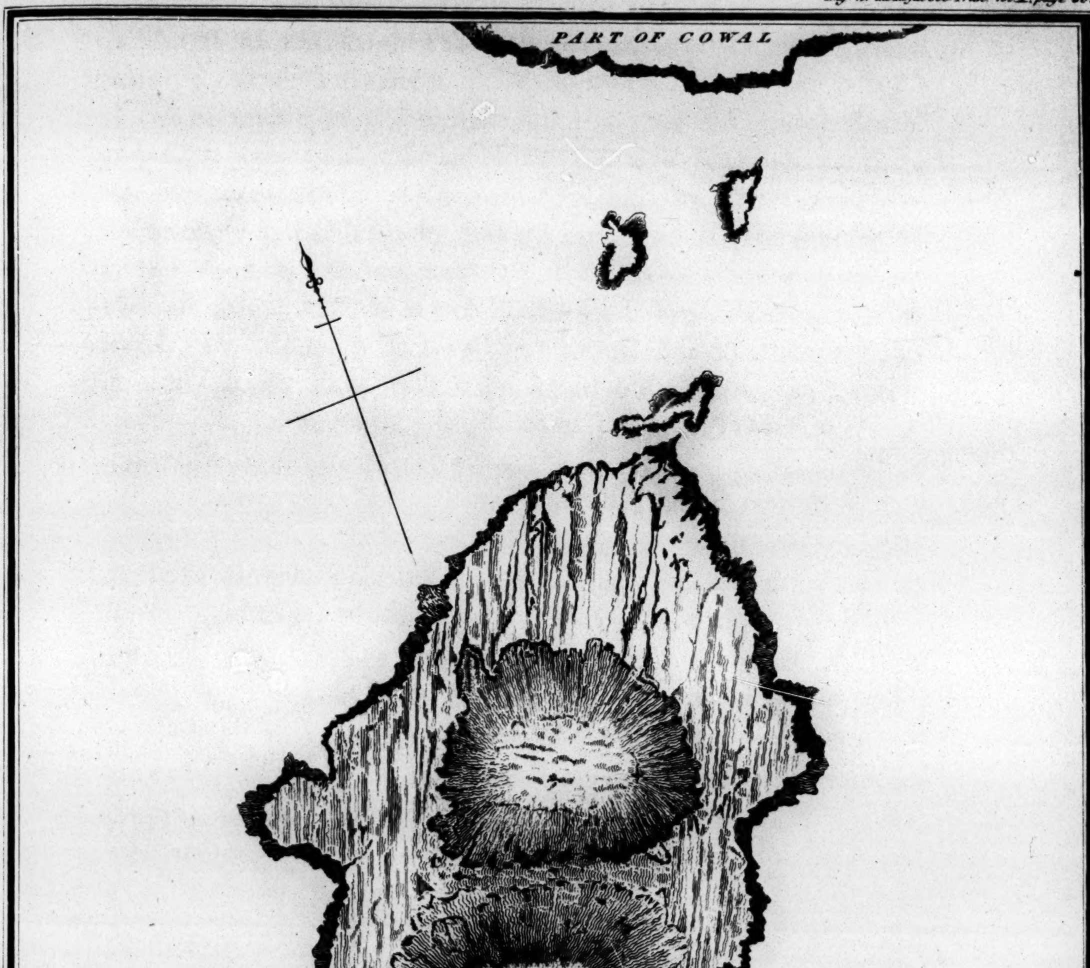
Within the walls the flat surface of the rock is exposed at A (Plate IV). Near it, at B, there is a small hollow, which was perhaps a well or cistern: at C there is an appearance of a ditch. which, if artificial, was probably intended to strengthen the defence on that side.

There are some peculiarities in the situation of this fort which appear to me decisive of the question which is still agitated whether the vitrification is the effect of accident or design. Those who advocate the former opinion have supposed that they were produced by ancient volcanoes,—by destruction by fire,—or more recently, by the repeated action of signal fires. It is quite unnecessary to say any thing here as to their volcanic origin; * and I think it proved by the experiments of Dr MacCulloch, that, from the intensity of heat required to melt the most fusible

* Since this paper was written, the theory of their volcanic origin has been revived by Dr Hibbert, in consequence of an examination of the Fort of Finhaven, in the county of Forfar. I have not seen that fort, but I cannot imagine that it will apply to the one in question, which is of a regular form, and placed on the flat surface of a primitive rock. Neither can it be accounted for by supposing that volcanic productions have been brought from a distance for the purpose of building, because both from the size of the vitrified masses, and from the downward direction in which the fused matter has run, we must conclude that the vitrification has taken place after the walls were built.

PLATE IV.

Engd for the Royal Soc. Trans. Vol. X. page 80.



of the rocks, it is impossible that any single conflagration could have produced such effects.

In an article in the ninth volume of the Edinburgh Encyclopædia, written, I believe, by Sir GEORGE MACKENZIE, these effects are attributed " to making signals by fires," chiefly because those hitherto known have been placed in commanding situations. I apprehend, however, that this will not account for the fort in question, because, in the first place, the situation, in a flat, surrounded on all sides by hills of considerable elevation, does not appear at all calculated for such a purpose; and, in the next place, the regularity of its form seems still more inconsistent with the effects of any accidental cause.

We must, therefore, I think, conclude, that, in whatever manner these singular buildings were constructed, or for whatever purpose, they are the effects of design. They were probably constructed at a period before the country was cleared of its original forests, when abundance of fuel, and ignorance of other modes of cementing stone, had induced the inhabitants to resort to the expedient of joining them by fusion.

IV. *On the Formation of Chalcedony.* By Sir G. S. Mackenzie, Baronet, F. R. S. Lond. & Edin.

(Read January 21. 1822.)

Although I possess a collection of specimens of Chalcedony, made expressly with the view to enable me to form some plausible theory of its formation; and, although my cabinet contains a variety of forms, perhaps unequalled in any other collection, I have, for many years, refrained from describing them, because the oftener I contemplated my specimens, the less able I found myself to apply any single agent, so that its operations alone could account for their forms. It is, indeed, very long since I entertained the opinion, that no hypothesis founded on the separate agency of heat or of water, could be satisfactory to speculative minds; and that geologists could never account for the formation of individual minerals, or of the rocks containing them, without combining the power of heat with that of water. But even this combination, though it can be easily conceived capable of forming many of the productions of the mineral kingdom, leaves numerous phenomena unexplained.

While we endeavour to account for the forms of Chalcedony, we are not fettered by the appearance of animal remains, as in the case of flint. It has indeed been supposed by some very ingenious philosophers, that Chalcedony incloses some very delicate vegetable forms; but, while we know that various substances belonging to the mineral kingdoms, some of which are found connected with Chalcedony, assume arborescent forms, we may

be permitted to hesitate to fix our belief, until we are admitted farther into the secrets of Nature.

It will be allowed, that all attempts to account for what we see in the material world, however unsuccessful they may be, are so many steps towards discovery. The failure of one leads to another. The naturalist is situate as if he were standing on a spot whence numerous roads diverge, but of which only one leads directly to Truth. He travels over many of them without having struck into the right one; but he gains at least the satisfaction of having reduced the number yet to be examined, and to have increased the chance of success to future adventurers.

The means by which Silica may be reduced into a condition, from which it may assume the forms of chalcedony, of flint, of crystals, or in which it may be introduced into the minute pores of animal and vegetable bodies, may long continue to be a mystery. We may hope, however, that the discovery of these means may yet exalt the name of some fortunate philosopher, who, with talent, observation, judgment, and perseverance, such as have displayed to an admiring world the nature of the alkalies and earths, will reduce the opacity of the veil that conceals some of the most important and most wonderful operations of Nature.

Geological theorists have, in their anxiety to generalise, been too prone to attribute all appearances to one proximate cause. WERNER looked only to one cause, the solvent power of water; but water refuses to perform what has been ascribed to its power. HUTTON regarded fluidity, caused by heat, and modified by compression, as sufficient to account for every thing, while it is speedily obvious to a careful observer, that the number of the phenomena which can be best explained by the agency of heat, is limited. The absurdities which have been published, for the purpose of accommodating the theory of aqueous solution to facts which bear the impress of a different process, are now ra-

pidly vanishing before the independence and candour of the pupils of the Wernerian School itself. Many of them have gained much credit to themselves, not merely for their careful examination of foreign countries, but from their having appealed to the very scenes of their education, and acknowledged that they now consider the views formerly impressed upon them to have been erroneous. There is yet, however, a farther stretch of candour to be expected from them; and that is an acknowledgment, that they had been anticipated in their views of the origin of Trap-rocks, by the result of volcanic and trap countries having been examined by geologists of the Huttonian School. The proofs of the igneous origin of such rocks, that were deposited in the cabinet of this Society more than eleven years ago, will now be regarded as of more importance than what was then conceded to them.

The idea of HUTTON, that the materials of all rocks have been so affected by heat, as to have become in some cases soft, and in most cases fluid, is, as a general proposition, manifestly contradicted by facts. His theory, however, as illustrated by PLAYFAIR, I have always considered as better fitted to explain a great mass of facts than any that has yet been promulgated. But it does not explain every thing; nor can I see any reason for appealing either to heat or to water exclusively, for the consolidation of the globe, while both agents may be called to our aid, and while the power of gravitation is in continual action. I have it in my power to satisfy the Society, that there is another power sufficient to convert loose materials into solid masses. I refer to the power usually denominated the Attraction of Cohesion. I had occasion, a considerable number of years ago, to fill a medicine-chest, that was to be sent into the country. Into one of the drawers I pressed some magnesia; but I cannot now recollect whether it was or was not then, as it is now, in the state

of carbonate. Some years afterwards I opened this drawer, and, to my great surprise, I found the magnesia as hard as chalk. I sent pieces of it from the country to my friends; but, from my having neglected at the time to commit the fact to writing, it has been forgotten, and the specimens have been lost.

I have, however, the satisfaction to find that a quantity yet remains in the drawer, which, though small, will be sufficient to demonstrate the fact, that the mere juxta-position of the particles of magnesia is sufficient for consolidating them. Gravitation, the attraction of cohesion, and the percolation of cold water, containing such substances as it is capable of dissolving, may account for the consolidation of many stratified rocks, though certainly not for that of all. Water greatly heated may affect much more as a solvent, and also by communicating heat. Nothing, however, in my opinion, can account for many other phenomena, but that the materials forming the rocks had been in a state of actual fusion. While we suppose actual fusion to have taken place, we may, without any risk of being considered fanciful, believe that water may have been present in greater or less quantity; and that, when strongly heated, it has the property of forming combinations, which it refuses to form when cold. Let geologists, therefore, as they are at liberty to do, call to their aid all the powers which nature presents, and cease to divide themselves into parties. They have but one object in view, the discovery of Truth. At present, geological theorists may be compared to an army advancing against a fortress, with nothing but balls on the right, nothing but guns in the centre, and nothing but powder on the left. The three bodies make separate and independent attacks; but the arm of each is useless singly. They must unite all the arms together before any impression can be made.

It is not my intention, in this memoir, to enter much into the consideration of causes. I propose to illustrate, as far as it may

be possible, by means of a few of the specimens I possess, the condition in which Chalcedony must have been, previous to its assuming the forms it now presents. This may appear to some a superfluous labour, as it is acknowledged on all hands that Chalcedony has been fluid. But the kind of fluidity has not been agreed upon. Besides, I trust that what I have to remark will not be altogether uninteresting. I have on all occasions stated my conviction, founded on proofs which I have seen, and examined over a large extent, and several of which are in the cabinet of this Society, that the rocks in which Chalcedony is most commonly found, are of igneous origin; that is, that the whole materials of which these rocks are composed have been placcd as we see them, by volcanic power acting under the influence of particular circumstances, and especially of pressure. I shall now endeavour, by an impartial examination of specimens, to ascertain whether the condition of Chalcedony, before its consolidation, was compatible with that conviction.

The various forms of Chalcedony may be comprehended, perhaps, in four general varieties, to which I may give names, without any other object than to facilitate description. I will therefore distinguish them by the appellations, Massive, Parallel, Botryoidal, and Pendulous. In the term Massive, I include the agate structure, and I use it merely, because, though all solid masses (with a very few exceptions *) have the agate form, that form is not always visible, till the eye is assisted by cutting and polishing the stone. By Parallel, I understand that variety which is combined with varieties of opal, &c. in straight layers, and which proves that these substances have had a common origin.

* I have but one specimen in which I cannot distinguish the agate forms even with the help of polishing.

The term Botryoidal is often applied to mineral forms, and is sufficiently understood. Pendulous I use in preference to Stalactitic, because the latter term implies a peculiar mode of formation already ascertained *.

No. 1.—The first specimen I exhibit is one in which the agate structure is not visible; it has a small cavity, lined with minute crystals of quartz, and in its general appearance resembles flint.

No. 2.—This specimen is also massive; and in its ordinary fracture the agate structure is not seen; but on the side from which a slice has been taken, that structure displays itself. This specimen is valuable on account of its exhibiting a peculiar fracture, which cannot be described by words. It also shows an exterior shell, such as I have succeeded in separating from another specimen of the same sort.

No. 3.—This shell has the botryoidal form, and has given the impression of this to the Chalcedony which has filled the cavity formed by it, and which, therefore, is of posterior formation.

No. 4.—In this specimen the parallel form is very slight, but sufficiently visible.

No. 5.—This shows the parallel and massive forms combined; and there is a minute cavity, lined with quartz-crystals, in the

* This Memoir is printed nearly as it was read to the Society; and there is unquestionably some awkwardness in the references to the specimens, while neither specimens nor drawings of them can be examined. With respect to drawings, it is exceedingly difficult to represent mineral bodies well, without great labour and expence; and the forms of the specimens referred to in this Memoir, as well as their general appearance, are such as render an attempt to represent them by engraving almost hopeless. Indeed it would be useless, as many of the specimens referred to can be viewed only by turning them in different directions, and placing them in different lights. The facts are stated in such a manner, as, it is hoped, will leave nothing equivocal in the mind of the reader.

centre. This specimen proves that both forms have had the same origin.

I possess a number of specimens which shew considerable variety in the parallel form, and some of which will be exhibited in connection with other forms. In some specimens, the lines appear as if the matter forming them had collected, or secreted into this form while the whole was fluid. I remember having observed some beds of trap in the Faroe Islands, which, when viewed at a little distance, had the same appearance in respect to the nodules of zeolite which they contained. The nodules appeared exactly as if they had been arrested in their progress towards forming lines, like flint in chalk, by the consolidation of the bed containing them.

I have exhibited a specimen of the massive Chalcedony, from which I had detached the exterior shell, which, impressing its form on the included mass, proves that the shell had been first formed, and had become solid, before the hollow, which it surrounded, had been filled up.

No. 6.—I now produce a specimen of a shell which has the botryoidal form, and which, from some remains of quartz-crystals, appears to have been lined with them. But on the Chalcedony we observe the impressions of crystals, which prove that the former was not, as in the other case, solid before other matter was introduced.

No. 7.—Sometimes the shell, and the Chalcedony filling the interior, are different both in colour and texture. In this specimen the shell is opaline and bluish white, and the interior massive Chalcedony is blackish grey.

The botryoidal form occurs in considerable variety of shape, colour and size. Its origin is to be attributed partly to the surface of the cavity containing it, but chiefly, I believe, to the Chalcedony having, in its progress into the cavity, assumed the spherical form, and to a number of such forms, while soft,

uniting. The pendulous form seems to be only an enlargement, or rather a prolongation, of the botryoidal; and, to illustrate this, I exhibit a beautiful specimen, having a pearly lustre. This lustre is given by zeolite, or the substance called Cachalong, forming the nucleus, or the thread, on which the Chalcedony has been formed. The connection of the botryoidal with the pendulous form is apparent.

No. 8.—I now exhibit a specimen, in which the massive and pendulous forms are united; and there are other specimens now on the table, which show all the forms in a small space. Hence, we may refer all the forms to one and the same previous condition of the substance of which they consist. That condition has evidently been fluid; but the kind and degree of fluidity is yet to be investigated. Before proceeding farther, I may exhibit the diversity of appearances which we meet with in the cavities, which are formed by an exterior shell of Chalcedony.

It often happens that we find nothing within the shell, which varies much in thickness; being sometimes scarcely thicker than paper, and in other cases several inches in thickness, of which several specimens are on the table.

No. 9.—In the next specimen there is imperfectly crystallised quartz within the cavity; and we observe the impressions of other crystals deeply inserted, but not penetrating to the Chalcedony. The crystals that have made their impressions were probably of calcareous spar.

No. 10.—Another specimen, although connected with the pendulous form (which is always contained in cavities) serves to shew a remarkable series of depositions. We observe, first, that a dark-coloured mass, probably a portion of the rock in which the Chalcedony was contained, has assumed the pendulous form. Around this, the Chalcedony forming the exterior shell of the cavity, has formed a pendulous mass. The next coating is formed of quartz, which, as it recedes from the Chalcedony, as-

sumes the crystalline form. The Chalcedony and Quartz are distinctly separated by a thin film of a pale-bluish colour. Over the quartz has been formed a layer of a white substance, probably opal or zeolite. Lastly, over this has formed a coating of quartz, which shoots its crystals into the cavity.

These successive, and varied formations may be supposed by some to have been produced by different fluids, introduced into the cavity at different times; and by others as a modification of crystallisation from one fluid. We have already seen the practicability of separating an exterior shell from the matter that has filled up the cavity. In the present case, the shell has assumed, as is usual, the botryoidal form; and there appears nothing to lead to the supposition that it had not become solid before the next layer, which is of quartz, had been formed. The quartz of the second layer appears, evidently, to have formed its crystalline points; and these points have caused the third layer of opal or zeolite to assume the botryoidal form. Now, it is not, perhaps, possible that, during the crystallisation of different substances from one fluid, any layer with the points of the crystals formed, except the last one (that in the interior of the cavity), could exist. On the other hand, it may be argued with equal plausibility, that, when once a hard shell is formed, covering every part of a cavity, it is impossible to conceive how any fluid could enter to form a second layer; and the difficulty of conceiving this becomes always greater, the more numerous and varied the layers become. Other arguments, on both sides, might be stated, and illustrated by specimens; but as these do not appear to preponderate more to one side than to another, I will not at present pursue the subject. I have said enough to prove, that it is no easy matter for us to comprehend the operations of Nature. As it is impossible for us to define the difference between that which is material and that which is immaterial, or to know any thing at all of the nature of matter, without knowing the essence of

things; so I believe it to be impossible for us to penetrate into the modifications which have obtained in producing the appearances we are now considering. We are satisfied of the existence of a certain order of succession, and we may live to see the cause which rendered the objects of our investigation fluid; but I apprehend we shall never understand the mode of action by which the properties and characters of mineral bodies are kept distinct, in situations and circumstances in which we might expect them to be blended together, so as to produce an anomalous mass *.

No. 11.—In order, however, to illustrate this subject still farther, I exhibit a specimen, in which the evidence of the quartz-crystals having assumed their proper form, before the substance deposited over them had become solid, is distinct. This substance appears to be opal or zeolite, as in the former specimen; and over it is a coating of very minute quartz-crystals; thus presenting an alternation similar, in order, to the other case, but having the crystallisation of the quartz more perfect.

No. 12.—Another specimen of this alternation, to which I request the attention of the Society, is a very remarkable one. The quartz-crystals are distinctly formed, and a slight coating of Chalcedony appears over them. But above this is deposited a white dust, some of which adheres firmly to the crystals, while the rest is loose. This suggests the idea, that the cavity had been filled with vapour, which had condensed into the form of this dust.

No. 13.—In the next specimen the shell is thick, and is lined with a soft white substance. On looking at these specimens, the advocates of the igneous theory may probably perceive something not unlike the process of Sublimation.

* Among numerous examples, well known to mineralogists, I may mention one specimen in my possession, not much larger than a man's fist, which is formed of distinct crystals of brown quartz, white topaz, and beryl.

The last specimen of this kind which I submit to the inspection of the Society, is that described by Mr ALLAN, in his account of the minerals of the Faroe Islands, and which is published in the Transactions. The peculiarity which this specimen presents is, that the coating of Chalcedony over the quartzy crystals appears to become thicker, as well as the crystals themselves larger, and the mass of quartz greater, as we examine from one end of the specimen to the other. The coating of Chalcedony has assumed the botryoidal form, and this gradually becomes more enlarged and distinct as the coating becomes thicker. The idea of the formation of this specimen, which may probably naturally occur on the first inspection, is, that the cavity, at the time when the quartz was introduced into it, was in a position having the larger end downwards; and that in consequence of this, the accumulation of quartz became greatest at that end. But let us remark, in the first place, that the outer shell of Chalcedony preserves a uniform thickness all round, except at the very extremity of the smallest end. It becomes, therefore, as necessary to account for there being no accumulation of Chalcedony at the lower end, as for there being an accumulation of quartz. This last is not such an accumulation as we should expect either from the entrance of a solution, or of a substance fluid *per se*. In the first case, if the entrance of the fluid had been gradual, we should have observed successive coats, or successive crystallisations; but here we have no succession of coats, nor of crystallisations; the crystals extend all the way from the shell to their points without interruption. I am aware that crystals are common, which show that they have been enlarged by successive depositions of matter; but it is not easy to conceive how such an enlargement could proceed, while the fluid was moving from one part of a cavity to another. As the accumulation of Chalcedony over the quartz appears, if I may so express myself, to have the same local ratio as to quantity with the

quartz, we may appeal to it for illustration. Let us suppose, then, the cavity placed with its two ends in the direction of the plummet, and fluid Chalcedony entering by the upper end, and obeying the law of gravity. In these circumstances, I cannot conceive how the fluid was conducted, so as to coat each crystal uniformly on all sides, and at the same time so that no more Chalcedony should rest on the lower than on the upper faces and angles of the quartz-crystals; nor can I account for there not having been a mass at the bottom with a horizontal surface. Thus, when we examine a specimen with minute attention, we find our first notions respecting it incorrect. With these remarks, I leave this remarkable specimen, as a proof of the danger of maintaining sweeping theories, that pretend to explain every thing. It has been asserted, that a philosopher sitting in his closet, with a specimen before him, cannot reason correctly on the mode in which a mineral has been formed; but the specimen we have been considering proves the contrary; and also defies him who may boast of having traversed every mineral mass that is exposed to view anywhere on the surface of the globe. We may be enabled accurately to ascertain general circumstances; but particular modes of action, and formation among mineral substances, under such circumstances, we can explain but seldom.

I now come to consider the Pendulous form of Chalcedony, which affords an ample field for the exercise of theoretical ingenuity; and at the same time proves the utter hopelessness of the task, when any one undertakes to find in it a demonstration of his theory, and attempts to discover the minute circumstances, and laws of action, which produced the forms which he studies. The forms we have already considered, do not afford sufficient data for assuming either the kind or degree of fluidity to which Chalcedony may have been subjected; but we shall find them in the pendulous form.

With respect to the size of the pendulous masses, I possess one in which it does not exceed the fiftieth part of an inch in diameter; and another in which the diameter is one inch and three-tenths; and I have seen some larger. In contemplating this great difference in size, we naturally recur to the state in which the substance was before it assumed its present form. We know that, from a small quantity of a very weak solution of a salt, very minute crystals will be formed; while, from an equal quantity of fluid more highly charged, larger crystals will shoot. But there is a limit to this; for after passing a certain extent in adding soluble matter, the resulting mass becomes confused. To prevent this, when we wish to procure large crystals, we use a large quantity of a rather weak solution, and allow it to evaporate slowly. But supposing, as some have done, that pendulous Chalcedony is a crystalline shoot, we cannot, I suspect, accommodate ourselves by supposing solutions of different strengths; and if we could, we see no means of carrying on a steady evaporation. But as we know of no such thing as a cylindrical crystal, we may at once set aside the idea of such a thing as chimerical. But it is not, perhaps, easier to account for this form of Chalcedony, with respect to difference of form in the pendulous specimens, by means of a state of fluidity from heat. From an inspection of the two specimens I have presented, a person, desirous of accounting for the difference of size, by an appeal to heat, as the sole cause of fluidity, would assert that the mass had been more fluid in one case than in the other; and that the minute forms owed their origin to a great degree of fluidity, while the Chalcedony forming the larger specimen had not been subjected to a greater degree of heat than was necessary to render it viscid. This would be very plausible, were these the only specimens we had to appeal to. But when we look farther, we find, in a small specimen, such as that which I now submit to the inspection of the Society, the pendulous masses varying very much in size,

and, which is most remarkable, the size increasing gradually from one end of the specimen to the other. We cannot imagine that the heat varied so much in intensity within the space of a few inches, as to render the Chalcedony in one part exceedingly thin, and in another viscid.

There is another supposition, with respect to this form, which may be considered, while the former is before us in its most simple shape. It has been suggested, that the pendulous form has been produced in the same manner as a stalactite, by the successive deposition of layers from a solution. This idea appears exceedingly plausible, when we find, for the most part, this form presenting concentric layers. But we know that such layers may be produced by simple fusion in particular circumstances. For instance, when a wax-candle is about to be made, the wick is suspended in the centre of the mould, and the melted wax is poured in at once. From such an operation we should not expect to find the candle formed of layers. Nevertheless, it is so; and by exhibiting such a candle, I can satisfy the Society of the fact. This appearance of concentric layers or coats, is evidently caused by the wick operating in some peculiar manner, so as to give a particular direction to the process of crystallisation. In almost all the cases in which the layers in Chalcedony are perceptible, we find something analogous to the wick of the candle; either a hollow tube, or a thread of chlorite, zeolite, or some other substance. In the only large specimen of pendulous Chalcedony which I possess that has not such a thread within it, no layers are perceptible, even with the assistance of cutting and polishing.

The strict parallelism of the layers of Chalcedony, in its different forms, cannot, I should think, be attributed to any other cause than crystallisation, under the influence of some modifying power; and such facts as I have stated, seem to favour the idea of a state of fusion, and to afford ground of triumph to its sup-

porters. But, the advocates of solution may say, unluckily for our opponents, we find cases in which threads of foreign matter have not only not produced a concentric arrangement around them, but have not interfered with the assumption of the parallel form. As it is my purpose to do justice to both sides of the question, I am bound to state that there is a most important difference between these two cases.—In that of pendulous Chalcedony, there is nothing to influence the crystallisation but the tube or thread, consequently all its tendencies are towards the centre. In the case of parallel Chalcedony, the matter has been acted on by the bottom and sides of the cavity in which it is contained, and this, together with its own gravity, seems to have exerted an influence superior to that of the foreign matter suspended in the fluid mass. In many specimens, the parallel Chalcedony seems to have been formed by the fluid matter rising from the bottom, as it gradually filled the cavity, and to have surrounded the pendulous matter previously formed. But, in the example before the Society, it is evident that the shape of the cavity has not exerted any influence in disturbing the production of the parallel form, as it appears to have done in the case of agates. Accident, however, has led me to the real structure of the Chalcedony in such cases. The specimen which I now submit for examination, was cut from a mass which seemed, as in the former specimen, to have surrounded the pendulous. The lapidary's oil having insinuated itself between the layers (a proof that the layers are not contemporaneous), when I heated the remaining portion of the specimen red-hot, the Chalcedony becoming opaque and white, the charred oil afforded a display of the actual structure; and it is certainly not such as might have been expected. On looking at a specimen in its natural state, we cannot predicate any other connection between the pendulous and parallel Chalcedony, than simple adhesion; yet we see how a mere accident can prove the uncertainty of our speculations.

The result of this accident shews a much more intimate connection than our eyes could before discover. It exhibits a process, the steps of which are so distinct, as to lead immediately to a mode of formation similar to the stalactitic. It appears that the first formed portion of the pendulous mass had reached lower than the subsequent coatings; and that the matter, in its liquid form, after depositing a coat on the pendulous mass, had spread out horizontally to form parallel layers. The extreme thinness of some of the coats indicates an extreme degree of fluidity; while the accuracy with which the right angle is preserved between the descending coat and the horizontal layers is very striking; and hence we may conclude (as well as from the uniform thickness of each layer), that the passage from fluidity to a state of solidity must have been rapid. This conclusion leads us to another, I think irresistibly, that Chalcedony has been fluid *per se*, and that it has become solid in the same manner as tallow, wax, or water when it passes into the state of ice. But before producing specimens to confirm that conclusion, I must point out another fact which the specimen now before us has brought to light. It appears distinctly, that the fluid matter has not only enlarged the pendulous mass, and thickened the mass at the bottom by successive descending coats, but that each coat or layer may be traced round the sides of the cavity; and, as might have been expected, there is a greater accumulation at the bottom than on the pendulous mass and sides of the cavity. Thus, no doubt is left of the mode of formation.

In the next specimen which I exhibit, we have some other curious facts displayed, besides a remarkable example of what I have been illustrating. In this we see the successive descent of the coats along the sides of the cavity. But it appears that a considerable quantity of Chalcedony had been previously introduced in some other manner, and which has assumed the parallel form at the bottom. The regular formation of the parallel form,

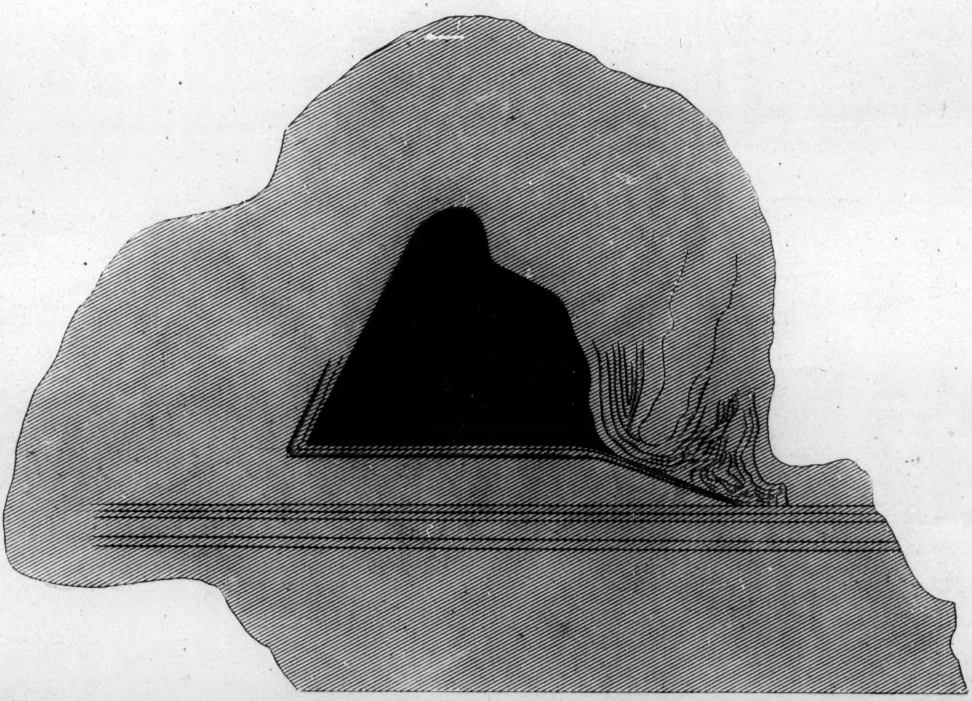
by the descent of successive coats along the sides, and their spreading over the bottom, appears in this specimen to have been interrupted, by the introduction of another substance in a fluid state, between which and the Chalcedony there seems to have been a repugnance, analogous to that manifested between oil and water. It has evidently not entered by the sides, nor along with the Chalcedony (at least so far as we can judge while we do not possess the whole geode), but it appears to have mixed with the Chalcedony after it had been introduced, and to have interrupted the regular formation of the parallel form, in a manner which cannot be explained.

Each successive layer of Chalcedony has insulated a portion of the white substance, and at the bottom we see a layer of quartz thus insulated. Though the regularity of the parallel form is interrupted, the connection of the successive coats is not prevented.

Before quitting this specimen, I have to remark, that at one side there appears some threads of foreign matter, around which the Chalcedony had formed in the pendulous shape, before that forming the interior mass had been introduced; and from the position of these, it seems as if that of the cavity had been changed. Another curious thing to be observed is, that the accumulation of matter from one of the side coats, immediately below the white matter (which is to all appearance zeolite), has included in it a number of white hemispherical bodies, which seem to indicate the incipient secretion, or rather separation of the substance which usually forms some of the layers of the parallel form. In another specimen, bodies similar to them appear to be formed of a number of concentric hemispheres. Such facts, however, though curious, do not lead to the principal object I have in view, which is, if possible, to demonstrate the kind of fluidity to which the matter forming Chalcedony has been subjected.

PLATE V.

Eng^d. for the Edin^r. Royal Soc. Trans. Vol. X. page 99.



W. H. Lizars sculp^t.

I will now exhibit a few specimens of the parallel form, which present facts altogether anomalous with respect to the laws which fluids in general obey. In the first, we see that the surface of the Chalcedony forming the undermost layer is not parallel with that of the uppermost, and the intermediate layer of opal consequently takes the form of a wedge. In the next, this kind of anomaly is still more remarkable, and cannot be explained by any effects which come under our ordinary observation *.

We observe in this specimen, that the upper part of the parallel form has been depressed in a singular manner by the matter forming a pendulous mass. It seems as if the depressed matter had disappeared; for, on the other side of the cavity, the coats follow the usual course. I confess that I am quite puzzled by this specimen. One thing, however, seems evident, that the matter had not been in a state of great fluidity.

I am now to shew a specimen which appears to indicate a state of viscid fluidity; or that state in which we observe glass when under the hands of the blower. In this we have the pendulous form thickened at the end, so as to resemble a drumstick. As the pendulous mass has come into contact with the matter below, we may either suppose that this has caused the thickening at the end, or that this shape had been assumed, as glass assumes it when dropt, and also thick honey. I cannot imagine this specimen to have been in any other state; and I have longed to possess another similar one, that I might examine the internal structure; for I cannot reconcile myself to the risk of losing that now before the Society, by attempting to cut

* Though in most cases it is exceedingly difficult to make intelligible drawings of the minute appearances which Chalcedony presents, a delineation of the fact now under consideration is easy, and it is given in Plate V. Fig. 1.

it, as it is interesting in another particular besides that on account of which I now exhibit it.

It is obvious that, if any change in the position of the pendulous form has taken place, the fluidity of the mass could not have been very great. I possess a great many specimens, the forms of which cannot be accounted for in any other way than by supposing that the cavities were in motion previously to the Chalcedony becoming solid. In one of these the pendulous masses have taken three directions. In another they appear as if radiating from a centre; and, in this one particularly, the form of a dropping mass of viscid glass is very striking. In a third we see the pendulous masses twisted in a variety of ways; some into the form of a cork-screw. In another cavity we have a few twisted, but the greatest number are all bent in one direction. The most beautiful specimen of this which I possess, shews the pendulous form, after being fairly bent round, attached at both ends to the side of the cavity. This has no appearance of two pendulous masses having met and joined. The curve is quite regular, and not in any degree altered by thickening, which would have happened had two masses met; nor is there the slightest indication of the matter having made farther progress downwards. Such a form as this could not possibly have been produced from a state of extreme fluidity; and hence there may be drawn a strong argument for fluidity by heat; and for pendulous masses having, in some instances, been formed at once, and not by a deposition of successive coats. But we are by no means allowed to rest satisfied with such a conclusion as a general one. I now present specimens which prove, most distinctly, the matter of which they are formed to have been as thin, perhaps, as water. Whoever has seen water trickling from above, amongst grass or trailing plants, during frost, will have observed a formation of ice precisely of the form which Chalcedony has assumed in this specimen; which also proves that Chalcedony has been

formed, not from a solution, but from matter fluid *per se*,—a fluid that has become solid as it advanced, in the same manner as water during frost; or, in other words, which has become solid by the deprivation of heat, and not by the evaporation of a fluid. Arriving at this conclusion, we must examine the effects of heat on other substances, in order to obtain an analogy which may reconcile all the forms in which we find the very interesting substance I have been considering.

I have already pointed out the fact, that wax, when poured into a candle-mould, arranges itself around the wick in the same manner as Chalcedony appears to do around the matter which we find in the centre of the concentric pendulous form. But what we have now to observe of wax is, that when it arrives at a certain temperature, it passes *suddenly* from a state of solidity, (I should say cohesion perhaps) to that of extreme fluidity; and its transition from fluidity to the solid state is equally rapid. To this substance, therefore, we may compare some forms of Chalcedony, in respect to their previous condition and formation.

Glass passes very slowly from the fluid to the solid state; and to this we may compare other forms of Chalcedony in the same manner. But the same thing cannot be similar in its properties to two dissimilar things; and we must, therefore, imagine, that some adventitious circumstances in the composition of Chalcedony, have had the power of altering its properties; and that these circumstances are too minute for us to discover. That very minute additions to earthy compounds greatly alter their figure and properties, we know by the facts brought to light by the labours of analysts; and we may hope that Chalcedony may yet attract their notice more than it has hitherto done.

No chemist, so far as I know, has observed water in Chalcedony, except KLAPROTH, who found in the green variety (Heliotrope) 2.5 per cent. of water. BERGMANN obtained 16 per

cent. of alumina in one specimen, and 2 per cent. in another, together with 11 per cent. of lime. May not such variations in composition cause variations in the effects of heat applied to Chalcedony?

It is evident that the degree of heat necessary for the fusion of Chalcedony, is not so high as we might be apt to imagine; for in the specimens I now present, we find that Chalcedony has covered calcareous crystals without altering their form. We know, from the experiments of Sir James Hall, the degree of heat necessary for the fusion of carbonate of lime; and from the specimens it is clear, that the temperature required for the fusion of Chalcedony, had not the power to melt carbonate of lime. If, on the other hand, we are to call in the aid of a solvent, these specimens prove that the solvent of Chalcedony could not act on carbonate of lime. But it is useless to enter into a maze of supposition. I shall only observe, that I am rather puzzled, when I see in one specimen Chalcedony covering calcareous spar, and also calcareous spar covering Chalcedony. This is an anomaly which, in the present state of our knowledge, cannot well be reconciled to contemporaneous formation, whether we suppose heat or a solvent, or both together, to have been in action. Nevertheless, I am disposed to consider the formation to be contemporaneous; and possibly when we come to be better acquainted with the power of water greatly heated, the mystery may not appear so great.

There is yet another mode of formation to which some forms of Chalcedony appear to have considerable analogy. I refer to the formation of pendulous masses from vapour, of which the best illustration is to be seen in the singular substance sometimes found in the retorts of gas manufactories. That substance presents to us the botryoidal as well as the pendulous form; a combination very common in Chalcedony. I have already produced a specimen in which the cavity is sprinkled with a sub-

stance in the form of dust or flocculi, which most probably had condensed from a state of vapour.

When Chalcedony is heated, splinters are thrown off with explosion ; a fact that indicates the conversion of some ingredient of its composition into vapour. The escape of a component part is indicated also by the chalcedony becoming opaque. It is most likely that the matter which escapes is water ; and, indeed, I have no doubt that water has had a chief concern in the formation of Chalcedony, and also of opal, and of all the varieties of zeolite. That heat has operated along with it cannot be doubted, since we possess unequivocal proofs of its having affected the masses of rock in which Chalcedony is for the most part found I possess one specimen of the rock through which the Chalcedony appears to have become solid, while it was collecting and passing through the rock, having taken the branching form of bloodvessels. This proves, that both the matter of the rock, and the Chalcedony, had been soft and fluid at the same time ; and this precludes the idea of the formation of cavities, in the first place, and the subsequent gradual infiltration of a solution, if a thousand other facts did not contradict it. The exclusion of heat as an agent in the formation of the crust of the Earth, while so many phenomena warranted its assumption, has greatly contributed to multiply absurdities in the aqueous theories of the Earth, while the exclusion of water has hindered HUTTON and his illustrator from rendering the igneous theory the most perfect that has appeared. It is probable that water heated under a powerful compressing force, will be found sufficient to explain many anomalies both of the aqueous and igneous theories. But, as I stated at the beginning of this memoir, geologists ought not now to limit themselves to any one of the numerous agencies of Nature. When geology was beginning to assume the form of a science, it was of advantage, in the excitement of research, that there should be theoretical parties, however absurd the word

party sounds in matters of science, in which its origin can only be found in ignorance. Our knowledge of the structure of the crust of the globe is now so extensive, that, in endeavouring to account for the mode of its formation, we should feel ashamed to limit our views to any single agent or process. With respect to minute modes of formation in regard to individual minerals, the description I have given in this memoir will, I trust, prove the inutility of considering them, until experiments shall have entitled us to speak freely. If it does so, one object I have had in view will have been accomplished.

V. *Notice respecting the Vertebra of a Whale, found in a Bed of bluish Clay, near Dingwall.* By Sir G. S. Mackenzie, Baronet, F. R. S. Lond. & Edin. In a Letter to Dr Brewster, Sec. R. S. Edin., &c.

(Read March 17. 1823.*)*

My Dear Sir,

The Vertebra which I now send to you, was found about two years ago, on the property of Mr Mackenzie of Hilton, in Strathpeffer, county of Ross; and that gentleman desires me to present this interesting relic of some cetaceous animal to the Royal Society of Edinburgh.

Some years ago, a navigable canal was cut from the mouth of the River Conan to the Town of Dingwall, and the operation displayed a bed of dark bluish clay, containing sea-shells in great numbers. The thickness of this bed I have not been able to ascertain. A successful attempt having been made to drain the lower part of Strathpeffer, which is now in a high state of cultivation, this bed of clay was found to extend several miles up the valley; and it was in clearing out the drain that the bone was found in the clay. It is probable that more of the skeleton might have been found, had search been made for it.

This bed of clay has evidently been formed at the same period with that which is seen along the south shore of the Forth, as it is about the same height from the present level of the water. The bone was found at a distance from high-water mark of about three miles; and the height above the sea, at Dingwall, of the spot from which it was dug, is about 12 feet. The date

of its deposition must be very remote; for the great mass of gravel, which you saw the rivers of this county cutting through, is a deposition subsequent to the clay which it covers; and in many places peat-mosses have been formed above the gravel. You may remember my pointing out to you two distinct alluvial deposites; one consisting chiefly of clay, covering the sides of the hills, and including enormous blocks of stone; and the other, consisting of gravel, which fills the valleys, and has been cut through at different periods, as the succession of terraces testifies. I have seen nothing that certifies the comparative ages of the marine clay and the oldest alluvion; but the latter I consider to be the oldest. It appears, therefore, that many centuries must have passed since the sea retired from Strathpeffer, or since the land was elevated. Whether the ocean be again advancing, I will not pretend to decide; but I have not yet subscribed to Mr STEVENSON's theory.

I am sure that the Society will be glad to preserve the bone, as its having been discovered in circumstances nearly similar to those under which the skeleton was found at Airthrie, and on the same side of the island, render it extremely interesting.

I am,

My Dear Sir,

Truly Yours,

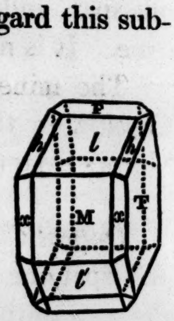
G. S. MACKENZIE.

COUL,
21st January 1823.

VI. *Description of Hopeite, a New Mineral, from Altenberg near Aix-la-Chapelle.* By DAVID BREWSTER, LL.D. F.R.S. Lond. & Sec. R. S. Edin.

(Read June 17. 1823.*)*

ABOUT the end of the year 1821, when I was engaged in the examination of the Family of the Zeolites, Mr HEULAND was so obliging as to send me a variety of Stilbite from a Calamine mine at Altenberg near Aix-la-Chapelle. Upon comparing its optical structure with that of the Stilbites, it was manifest that it had no connection with this class of crystals, and that it constituted a new mineral species. Upon mentioning this result to Mr BROOKE, this acute mineralogist was of opinion that it was the Silicate of Zinc. Mr HEULAND had been led to regard this substance as a Stilbite, in consequence of having received it as such from Major PETERSEN; but, particularly, from finding in the collection of Mr C. H. TURNER, a single crystal of the same substance attached to Carbonate of Zinc, and bearing the annexed figure, with an inscription in the handwriting of the Abbé HAUY, stating it to be a new variety of Stilbite, to which he gave the name of *Stilbite Duovigesimale* *.



* Attached to this figure is the following memorandum in HAUY's handwriting: *La position de cette figure est en rapport avec celle du noyau dans les planches du Traité.*

Having determined, by many physical experiments, that it was not Silicate of Zinc, I was anxious to obtain as much as would be sufficient for the purposes of analysis. Mr HEULAND exerted himself with his usual zeal for science, to obtain additional specimens; but Major PETERSEN, to whom he applied for this purpose, informed him, that there was reason to believe that no specimens had been found excepting those which Mr HEULAND had already received.

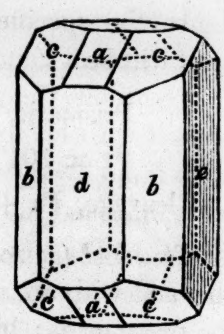
As those specimens were insufficient for chemical analysis, it became necessary to examine the mineral by means of other methods of observation, by which its existence as a New mineral species was completely established.

Description of the Mineral.

Physical Character.—The specific gravity of a perfect crystal was 2.76, and that of a larger specimen, with some black metallic particles adhering to it, was 2.91. It is scratched by calcareous-spar, and its hardness is consequently below 3.0 of MOHS's scale. It is neither phosphorescent nor electric by heat.

The mineral must therefore be placed beside *Anhydrite* and *Cryolite*, in the 1st order, or that of *Haloide* of the 2d class of MOHS's System.

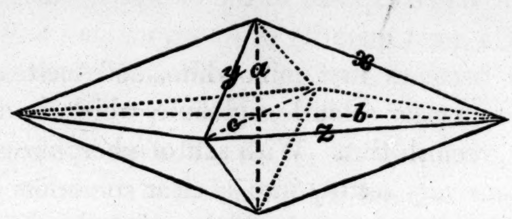
Optical Character.—It has two axes of double refraction, the principal one of which is perpendicular to the axis of the prism, and also to the planes of most eminent cleavage. The action of this principal axis is *negative*, like that of calcareous spar; and the inclination of the resultant axes of double refraction, measured with as much accuracy as the specimen would permit, is about 48°. The index of the ordinary refraction, through one of the summit planes, and one of the faces of the prism, was nearly 1.601.



Crystallogruphic Structure *.—The perfect crystal of this mineral is shewn in the annexed figure. The angles taken by the reflecting goniometer were,

$$a \text{ upon } a \;=\; 78^\circ\, 36'$$
$$b \text{ upon } b \;=\; 81\;\; 34$$

If we suppose the faces *c, c, c, c* enlarged till all the others disappear, the crystal will assume the form of a scalene four-sided pyramid (as shewn in the annexed figure) whose edges have the following values:



$$x \;=\; 107^\circ\, 2'$$
$$y \;=\; 139\;\; 41$$
$$z \;=\; 86\;\; 49$$

Hence the ratio of the axes will be

$$a : b : c = 1 : \sqrt{4.443} : \sqrt{1.493}.$$

As the form resulting from the enlargement of the faces *c, c, c, c* is the fundamental one according to Mohs's System, and designated by P, we obtain, by taking the angle of the horizontal prism at the apex of the pyramid, $a = P\bar{r} = 101^\circ\, 24'$, and $b = (P\breve{r} + \infty)^3 = 81^\circ\, 34'$, the angle of the vertical prism corresponding to the obtuse angle of the base of P. The mineral has two cleavages perpendicular to each other; the one parallel to *d*, which is easily obtained, and distinguished by its pearly lustre; and the other parallel to *e*, which is less distinct.

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* I have been indebted for this crystallographic description of the mineral to Mr Haidinger.

Hence the specific character of the mineral, in relation to its form, will be

Prismatic, $P = 139° 41'$; $107° 2'$; $86° 49'$

$\bar{Pr} = 101° 24'$ $(\breve{Pr} + \infty)^3 = 81° 34'$

Cleavage, $\bar{Pr} + \infty$ perfect, eminent. $\breve{Pr} + \infty$ imperfect.

Chemical Character.—It is entirely soluble without any residue, and without swelling or effervescence, in the muriatic and the nitric acids; but more rapidly in the former than in the latter. It is acted upon very slowly by sulphuric acid, whether strong or diluted; but is finally dissolved by it.

When exposed to the blowpipe * alone in the matras, it gives off a great quantity of water, without any trace of carbonic acid. It becomes first milk-white, and melts afterwards very readily into a clear colourless globule, which, in melting, gives the flame a greenish tint. With salt of phosphorus it melts in all proportions very readily into a clear colourless glass, no skeleton of silica being observed. If the mineral is in great proportion to the salt of phosphorus, it turns opaque in cooling, but no fumes of zinc are condensed on the charcoal †. With borax it fuses into a clear colourless glass, which does not turn opaque in cooling. It gives with soda a scoria, which, when hot, is of a yellowish colour. The oxide of zinc is condensed round it in great quantity upon the charcoal, and nearest to the scoria is a reddish-yellow tint, which does not vanish in cooling, and indicates the presence of Cadmium. When melted with soda, and moistened, it exhales no hepatic odour. A solution of cobalt communicates a fine bluish tint to the melted fossil.

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* This examination of the mineral with the blowpipe was made for me by M. NORDENSKJOLD, an able chemist from Abo, during his visit to Edinburgh.

† With all the siliceo-carbonates of zinc, when melted with salt of phosphorus, in such a quantity that the glass turns opaque in cooling, a very perceptible ring of oxide of zinc is condensed on the charcoal.

This mineral seems, therefore, to be a compound of some of the stronger acids, such as phosphoric or boracic acid with zinc, mixed with an earthy base and a little cadmium.

Having thus established the existence of this rare and interesting substance, as a new mineral species, I avail myself of the privilege of giving it the name of Hopeite, in compliment to our learned Vice-President Dr Hope, who, to many general claims of having his name associated with mineralogical science, adds one more particularly memorable in the annals of this Society, namely, the discovery and examination of the New Earth of Strontites.

VII. *Astronomical Observations made at Paramatta and Sydney.* By his Excellency Sir Thomas Brisbane, K. C. B. F. R. S. Lond. & Edin. & M. Rumker. In a Letter to Dr Brewster, Sec. R. S. Edin.

(Read November 3. 1823.)

My dear Sir,

I request you will do me the favour of submitting to the Royal Society the accompanying observations made at Paramatta and Sydney. Those of the Comet, together with the elements inferred from them, are exclusively Mr Rumker's, to whom it is impossible for me to give adequate praise, either for zeal, assiduity, or intelligence. In order to have a better chance of observing the transit of Mercury over the Sun's disk, I proceeded to Sydney, about 15 miles from hence, and I am happy to say that we had a most propitious day throughout for the observations, and that the near agreement of them at both places tends to confirm their accuracy, as well as to increase their value. Respecting the observations of the Solstice, I have bestowed my utmost pains, and I trust they may be considered as worthy of a place in the Transactions, as among the first fruits of the Founder of an Australian Observatory to his countrymen; and when I acquaint you, that nearly ten thousand stars of La Caille's Catalogue have been observed here, and compared with his catalogue, by Mr Rumker, in which arduous work I have contributed but a very small share, I flatter myself that such information will be received with more than common interest by the Members of the Society, and that the

Observatory will be considered in a tolerable state of activity, when so much has been accomplished in eight months. You are aware, however, that the necessary reductions will occupy some months, as every clear day augments the catalogue. I think I may most safely pronounce this to be a very favourable climate for the astronomer.

I remain,

Most faithfully and truly yours,

THOS. BRISBANE.

P. S.—I omitted to mention in its place, in justice to Mr JAMES DUNLOP, with whose merits you are well acquainted, that it was he who first discovered the Comet of ENCKE with a sweeper, at which occupation, besides many others, I find him most valuable and useful.

Government House, Paramatta,
N. S. Wales, 17*th January* 1823.

Elliptic Elements of the Comet in September 1822.

Time of passing the Perihelion,	Oct. 24^D.221201.	Mean Time at Paramatta.
Long. of Perihelion upon the Orbit, -	271° 36′ 18.3″	} from mean Equinox. 1822.
Long. of descending Node, - -	272 42 23	
Inclination, - - -	52 40 41	
Logarithm e, - -	9.9966440	$\varphi = 82° 53' 11''$.
Logarithm $\frac{1}{2}$ Parameter, -	0.3585731	
Sidereal Revolution, - -	663554.3	Days.

The agreement between the elements and the observations will be seen from the following Table:

1822.	Mean Time at Paramatta.	Mean Longitude.	Errors of Elements.	Mean Latitude.	Errors of Elements.
Sept. 23.	8h 9′ 0″ 5	241° 53′ 30″	0″	24° 10′ 43″ N.	0″
24.	8 0 47.4	241 55 4	— 27	23 2 20	— 6
26.	7 50 20.3	241 57 1	+ 45	20 50 45	— 11
27.	9 15 4.1	241 59 41	+ 16	19 43 38	— 2
29.	8 42 0.1	242 4 0	+ 17	17 43 29	+ 8
30.	7 43 28.2	242 6 48	— 4	16 47 42	+ 1
Oct. 12.	7 44 51.1	242 47 8	+ 5	6 54 45	+ 7
16.	7 44 17.5	243 1 57	+ 86	4 12 3	+ 12
17.	7 49 35.6	243 7 49	+ 86	3 33 47	+ 151
21.	8 11 13.3	243 24 56	— 12	1 6 18.6	+ 6
22.	7 28 28.0	243 28 57.6	0	0 32 29.5	0
26.	7 48 14.3	243 47 4	— 5	1 42 36.7 S.	— 48
27.	7 46 6.8	243 51 17	— 43	2 14 57	— 68
28.	7 23 7.5	243 55 52	— 7	2 44 59	— 1
29.	7 24 1.1	244 0 59.8	— 39.8	3 15 57.2	+ 9
30.	7 30 20.2	244 4 56.0	0	3 46 47.5	+ 9
Nov. 2.	7 54 38.2	244 19 7	— 10	5 17 6.5	— 8
4.	7 43 21.7	244 28 26	— 6	6 14 36	— 4
7.	7 41 41.9	244 42 52	— 10	7 38 51.5	— 8
8.	7 45 59.1	244 47 58	— 25	8 6 8	+ 9
10.	7 47 49.5	244 57 12	+ 8	9 0 58	— 27
11.	7 45 54.0	245 1 56	+ 18	9 27 18	— 6

Transit of Mercury over the Sun's Disc, November 3. 1822.

Immersion of preceding limb,	23h 20′ 53″.95	Apparent Time at Paramatta.
Complete Immersion, -	23 23 34 .53	
Emersion of preceding limb,	2 5 23 .03	
Complete Emersion, -	2 8 21 .55	

Immersion of preceding limb,	23h 5′ 23″.32	Mean Time at Sydney.
Complete Immersion, -	23 8 6 .38	
Emersion of preceding limb,	1 50 1 .78	
Complete Emersion, -	1 53 0 .29	
Immersion of preceding limb,	23h 21′ 37″.97	Apparent Time at Sydney.
Total Immersion, -	24 21 .03	
Emersion of preceding limb,	2 6 16 .43	
Total Emersion, -	2 9 14 .94	

Winter Solstice 1822.

1822.	☉'s true Zenith Distance.	Reduction to the Solstice.	Correction for ☉'s Latitude.	True Zenith Dist. of Tropic of ♑
Dec. 14.	10°36′54″.25	16 4. 8	+ 0.22	10° 20′ 49″.67
15.	33 19.50	12 26.09	+ 0.10	53.51
17.	27 23.51	6 32.31	— 0.18	51.02
18.	25 3.12	4 17.63	— 0.33	45.16
19.	23 32.54	2 31.07	— 0.49	60.98
20.	22 7.98	1 12.75	— 0.61	54.62
21.	21 14.92	0 22.72	— 0.74	51.46
22.	20 54.46	0 1.01	— 0.85	52.60
23.	21 4.46	7.63	— 0.93	55.90
24.	21 33.61	42.56	— 0.94	50.11
25.	22 35.51	1 45.78	— 0.94	48.79
27.	26 12.28	5 17.02	— 0.82	54.44
28.	28 40.41	7 44.98	— 0.68	54.75
			Mean,	10 20 52.54
Solar Nutation,	-	-	- -	+ 5.67
				10 20 58.21
Reduction to January 1. 1823,		-	-	101
Mean zenith distance of Trop. of ♑ Jan. 1. 1823,				10 20 58.22
Ditto of Trop. of ♋ Jan. 1. 1823, by summer solstice,		-	-	57 16 25.697
Half Difference = obliquity 1823,		-	-	23 27 43.738
Half Sum = latitude of Observatory,			-	33 48 41.958

We add here the observations of the Comet of ENCKE, already transmitted in a former letter *.

1822.	Sidereal Time at Paramatta.	Mean Right Ascension.	Mean Declination.
June 2.	$10^h\ 39'\ 25''$	$92°43'51''.3$	$17°39'46''.3$ N.
3.	11 — —	93 46 20.7	16 53 7.5
4.	11 3 0	94 46 0.0	16 4 36.7
6.	11 7 38	96 42 11.6	14 22 42
7.	11 3 10	97 38 15.0	13 26 5
8.	11 17 25	98 33 47.7	12 31 18.6
10.	11 20 0	100 24 43.8	10 29 49.5
11.	11 24 39	101 19 44.5	9 26 46
12.	11 40 2	102 17 52.0	8 18 30
13.	11 42 4	103 15 2.0	7 6 30
14.	11 55 0	104 15 40.0	5 52 27
15.	11 40 48	105 17 0.5	4 33 40
19.	12 13 38	109 54 36.4	1 29 43.7 S.
20.	12 16 53	111 14 26.9	3 14 29.1
22.	13 18 46	114 12 20.5	7 8 ——
23.	12 53 55	115 47 41.7	9 9 48.4

* The Letter here referred to never reached the Secretary, otherwise it would have accompanied the present communication.

VIII. *On a Remarkable Case of Magnetic Intensity of a Chronometer.* By GEORGE HARVEY, Esq. M. G. S. M. A. S. &c.

(Read November 17. 1823.*)*

A BOX Chronometer having lately come into my possession, exhibiting remarkable proofs of strong and active Magnetism, I was induced to examine it particularly, and to ascertain the intensities of its different parts, by means of an apparatus resembling that employed by COULOMB, and which was capable, from its very delicate construction, of indicating the existence of the minutest traces of attraction.

By denoting the power of the terrestrial magnetism by 100, the intensity of the Chronometer, one inch above the centre of its crystal, was only 90.79, when the hour of XII pointed to the north; but, on turning the time-keeper, so as to bring IX into the same direction, the intensity was augmented to 102.29, the position of the oscillating cylinder remaining unchanged; and by again turning it another quadrant, so as to bring VI to the north, the intensity again declined to 90.69, corresponding very nearly with the result determined in the first position; and, lastly, when III was brought into the same situation, the measure of the intensity farther declined to 78.89: So that the attraction was a maximum when IX was directed to the north, and a minimum when III was brought into the same situation; and what is farther remarkable, the nearly equal intensities corresponding to the positions of XII and VI, approach very closely to a state of equality, with the mean of the maximum and minimum intensities.

The results, however, may be more conveniently examined in a Table.

North.	Intensity.
XII	90.79
IX	102.29
VI	90.69
III	78.89
Mean	90.66

When these conclusions were obtained, the Chronometer had not been in motion for some months. The time at which it stopped being $9^h\ 7^m\ 50^s$, will of course determine the respective positions of the hour, minute, and seconds hands, with respect to the oscillating bar, or the magnetic meridian. These particulars became necessary to be attended to, in consequence of the strong polarities of the three hands.

In two subsequent experiments, XII being directed to the north, the intensity at three inches above the crystal was 94.70, and at five inches 97.42.

Finding that such remarkable changes of intensity resulted from merely turning the Chronometer, similar experiments were performed at the same distance above the middle of the bottom of the box, and the results of which are recorded in the following Table,

North.	Intensity.
XII	77.17
IX	91.34
VI	101.26
III	94.94
Mean	91.18

and from which it appears, that the maximum intensity was found when VI pointed to the north, and the minimum when XII

was in the same situation; and that the mean of the four intensities approaches very nearly to an equality with that entered in the former Table.

The top and bottom of the Chronometer presenting so many varieties of attraction, it was conceived that similar anomalies might result from an examination of its sides. Accordingly, when XII was uppermost, and the oscillating cylinder one inch above the middle of the side, the intensity amounted to 105.61; but when the time-keeper was turned, so as to bring IX below the cylinder, the measure of the attraction rapidly declined to 89.61; and when VI was examined, it increased to 91.78; and, lastly, when III pointed upwards, it again declined to 84.05. These results may be conveniently arranged in a Table.

Side of the Chronometer uppermost.	Intensity.
XII	105.61
IX	89.61
VI	91.78
III	84.05
Mean	92.76

The preceding observations having been made on the external parts of the Chronometer, the intensity of its internal works was next determined; and, first, by placing the centre of the oscillating cylinder one inch above the extremity of the steel arbor of the fusee, which possessed magnetism in a very high degree, when the intensity was found to be 109.09; but when the measure of the attraction was ascertained, in the line of a common tangent, proceeding from between the barrel and fusee, XII being uppermost, it declined to 107.82. A still greater declension was, however, remarked, when the Chronometer was turned another quadrant, so as to bring the middle of the side of the

spring box an inch below the centre of the oscillating bar, IX being upwards, the intensity amounting only to 92.22; the north pole, at the same time, dipping three degrees. Three coils of the steel-chain were wound round the box. The time-keeper being afterwards moved through a third quadrant, so as to bring the cylinder over the spring of the balance, VI being uppermost, the north pole dipped two degrees, and the intensity amounted to 101.26. And, lastly, by turning the Chronometer through a similar portion of a circle, bringing III upwards, and by this means placing the centre of the oscillating cylinder over the small interval between the balance and the fusee, the intensity fell to 79.51. Below the cylinder, in this case, were the arbor of the fusee, and a ratchet and pivot for the same, all of steel, and possessing considerable magnetic power. These results are arranged in the following Table:

Side of the Chronometer uppermost.	Intensity.
XII	107.82
IX	92.22
VI	101.26
III	79.51
Mean	95.20

These conclusions bear some analogy, as, indeed, they ought, to those recorded in the preceding Table, the maximum intensity corresponding in each case to the position XII, and the minimum to that of III. The positions denoted by VI and IX are also in both cases next to the maximum in point of magnitude.

The magnetism of the balance and its spring were also powerfully displayed, by raising the Chronometer when VI was uppermost, so as to bring the circumference of the latter within an inch and a quarter of the oscillating cylinder; the dip being in-

creased from two to five degrees, and the intensity diminished from 101.26 to 95.99.

On examining the balance, the inner rims of the arcs of compensation were found to be of steel, and so likewise were the time-screws, which connected them with the transverse arm. These parts were in a state of active magnetism, particularly the time-screws, one having strong northern polarity, and the other southern. The small wormed cylinders also, on which the thermometer pieces moved, presented equal proofs of polarity, one being a north pole, and the other a south. The time-screw and thermometer piece having northern polarity, were on one side of the balance, and those having southern on the other. The balance-spring likewise exhibited vigorous polarity.

When the north pole of a small bar magnet was placed near the extremity of the wormed cylinder which possessed northern polarity, the balance immediately receded a small quantity; but when the south pole was applied, the power was sufficient to cause it to advance through a minute but sensible arc; and similar effects were produced when the proper poles of the magnet were presented to the extremity of the wormed cylinder having southern polarity. On presenting a more powerful magnet, the balance was drawn more than a quadrant from its quiescent position, and motion communicated to the Chronometer.

By placing the time-screws in the direction of the magnetic meridian, and bringing the north pole of a pocket-compass near that which possessed southern polarity, no deviation was of course perceptible in the compass-needle; but when the balance was moved through the arcs recorded in the first column of the following Table, the deviation in the direction of the compass amounted to the quantities entered in the second; the inertia of the needle being too considerable to admit of its inversion. By employing a needle of a more delicate construction, an inversion of its poles took place, the moment the time-screw had

passed through an arc of 90°; when a deviation of the south pole immediately followed.

Degrees of the Arc of Compensation.	Deviation of the Compass Needle.
0°	0°
10	$4\frac{1}{2}$
20	$8\frac{1}{2}$
30	12
40	$15\frac{1}{2}$
50	19
60	25
70	30
80	$36\frac{1}{2}$
90	$43\frac{1}{2}$
100	$49\frac{1}{2}$
110	$54\frac{1}{2}$

When the Chronometer was so placed that the transverse arm, which bears the time-screws of the balance, became east and west, a fine compass-needle, having its centre over the middle of the balance, immediately disposed itself in the same direction, its north pole reposing over the screw which possessed southern polarity. When also the balance was turned through an arc of 90°, the needle turned with it, the north pole in consequence pointing to the south. The moment, however, the balance was allowed to vibrate, the needle commenced its oscillations, vibrating in progressively decreasing arcs, from the first semicircle described by it, to zero in the magnetic meridian, where it maintained a small tremulous motion. In another experiment, when the balance was turned through a greater arc than a quadrant, before motion was communicated to the Chronometer, the needle was nearly inverted, the north pole pointing west; and on motion being given to the balance, the needle ranged for many seconds through the complete circumference, until the directive power of the earth, by gaining the ascendancy, caused the arcs of vibration successively to diminish, the needle ultimately ob-

taining a position coincident with the meridian, where it continued in a state of tremulous motion as before.

The quantity of steel contained in this Chronometer was truly remarkable, and no part of it was destitute of vigorous polarity. Every screw displayed its influence, and of which there were ten large, and several small ones, in the frame alone. The chain also, the axles of the different wheels and pinions, the arbor of the fusee, the balance and its spring, exhibited the same intense and active power *. Nor did this polarity partake of the transient character of that imparted by induction from the earth to soft iron, but was permanent, undergoing no sensible alteration from change of position.

From the short time the Chronometer has been in my possession, no satisfactory account has been obtained of its rate. During the three preceding years it was constantly on shipboard, and its general character is said to have been good; although, at times, it appears to have been subject to rather more than ordinary aberrations. It would be interesting if a few facts connected with its previous history could be obtained, as they might probably throw some light on the source from whence it derived its active magnetic powers. At a future time, I hope to be able to communicate something on this head.

On a subsequent occasion, another Chronometer was examined by means of the same apparatus. The balance evinced no proofs of polarity when small magnets were presented to it; but the apparatus of Coulomb detected some minute varieties of attraction in different parts.

* Mr Cox, the agent for Arnold's Chronometers at this place, and whose accurate knowledge of the principles and action of time-keepers is so well known to many of the most distinguished officers of his Majesty's Navy, remarked, when the Chronometer under consideration was shewn to him, that it appeared nothing less than a *Magazine of Magnets*.

By placing the oscillating bar three quarters of an inch above the centre of the glass-crystal, the magnetic intensity was found to be 94.36; and at the same distance above the centre of the bottom of the brass-case, it amounted to 100.63. When the Chronometer was turned so as to bring its side below the bar, III being upwards, the intensity was 98.51; and on determining it on the opposite side, or when IX was uppermost, it amounted to 94.02. When the cover of the Chronometer box was closed, and the intensity determined at the before-mentioned distance above its middle, the measure of the attraction became 99.13. The intensity therefore was the greatest near the bottom of the brass-case.

This chronometer appeared to have been constructed with every possible care, to avoid the introduction of magnetism. The handles of the box, the hinges and screws, the lock, staples and key, were all of brass; still, from the anomalous results above presented, the variations of intensity were more considerable than could have been anticipated, considering the very small quantity of steel that appeared in it. These variations are indeed inconsiderable when compared with the changes of intensity exhibited in the former instance; but are sufficient to prove that magnetism exists in Chronometers, when, from the precautions employed in their construction, we should have imagined it altogether removed.

That the application of magnets to Chronometers does not in all cases communicate magnetic qualities of a very powerful kind, may be inferred from the example of another Chronometer, which had been frequently employed in inquiries connected with magnetism, for many months, and which was subsequently examined by the apparatus of COULOMB. The oscillating cylinder was placed one inch above the crystal, and the intensity determined in four positions of the time-keeper, namely, when its XII o'clock mark was directed successively to the four cardinal points

of the horizon; the experiment agreeing in this particular with one determined for the first Chronometer. The following Table contains the results:

North.	Intensity.
XII	100.13
IX	99.75
VI	97.54
III	98.03
Mean	98.86

and from which it appears, that the mean of the four intensities approach very nearly to the assumed terrestrial intensity, and that, moreover, a much greater uniformity exists among the results than in those determined for the first Chronometer.

The examples that have been furnished by Messrs VARLEY, FISHER, BARLOW and Captain SCORESBY, relative to the magnetism of Chronometers, and from many experiments I have had an opportunity of performing, and a detailed account of which I hope soon to draw up, induce me to believe, that nothing short of the absolute removal of every thing capable of retaining the magnetic influence in the balance, will prove an effectual remedy for the errors to which the rates of Chronometers are liable from this cause. Captain SCORESBY, in an excellent paper published in the 9th volume of the Edinburgh Transactions, proposes, with his usual ingenuity, to free the balance from any magnetism it may have acquired, by causing it to be ground and polished in the plane of the magnetic equator; or, as they are now generally constructed of soft steel, to have them *turned* in that plane. This method of obviating their anomalous action, would in all probability be effectual, if similar precautions could be taken with the steel employed in the other parts of a Chronometer. But the chain alone would be capable of imparting, in a short

time, any magnetic qualities it might possess to the balance, and thus to restore to it that power of derangement which had been previously removed. Suppose, for example, that the balance of the first Chronometer alluded to in this paper were to have its magnetism removed by the ingenious method recommended by Captain Scoresby, or by any other, is it not probable that the same property would be again acquired, from the active magnetism possessed by the numerous screws, the arbor of the fusee, the chain, &c. in consequence of the balance either remaining quiescent, or incessantly performing its vibrations in the neighbourhood of that which may, without impropriety, be denominated a System of Magnets? On the whole, therefore, the employment of a substance in the construction of the balance, not only without magnetism, but without the capability of acquiring it, will be the only effectual and perfect remedy for the anomaly in question. Platina, or an alloy of platina, has been mentioned by the intelligent and active philosopher last alluded to; and it is not improbable, but that it may be ultimately found as well adapted to the purposes of compensation as steel. Similar precautions are necessary in the formation of the balance-spring. Gold, it is said, is very well adapted for this purpose.

Plymouth, *August* 12. 1823.

IX. *Remarks concerning the Natural-Historical Determination of Diallage.* By W. Haidinger, Esq.

(Read November 3. 1823.)

The following paper contains the results of a series of inquiries, which lead to the conclusion, that the mineral called *Smaragdite* by Saussure, does not form a species of its own; but that this name has been given to a compound of certain varieties of two distinct species, *Augite* and *Hornblende*, the natural-historical species of *paratomous* and *hemiprismatic Augite-spar*.

Owing in part to the slight degree of resemblance prevailing among its varieties, the authors who have described them differ so essentially in opinion, that I am obliged to go into various details, both respecting the external appearance of the mineral itself, and of the opinions of mineralogists, in order to afford a correct view of the natural-historical species, to which these varieties belong, since this is the basis upon which every system, and, indeed, all accurate information in natural history, is founded, and the fixed point to which the one and the other must be referred.

The more particular objects of these examinations are the grass-green, and such other green and greenish-grey varieties as are found to be in immediate connexion with them in the same identical specimens. A connexion between the real Smaragdite and the foliated Anthophyllite of Werner, or the Schiller-spar of the Hartz, does not exist, and has been gratuitously assumed by the Abbé Haüy, and by other mineralogists.

There is scarcely any thing which tends more evidently to prove the homogeneity of individual bodies, than the transitions between them, if correctly ascertained; but we must not

take for granted as a genuine transition, whatever appears to be so to the unassisted senses, or, indeed, what is considered as such in many mineralogical works. A real transition should not be considered to take place, till the differences in each of the single characters, taken separately, have been collected into as many continuous series *; and before this has been accurately effected, it will in every respect be found more useful for natural history, to consider as particular, although doubtful, species, such varieties as are but imperfectly known, than to unite them with other species, respecting which our information is more complete: for the uncertain existence of a particular species, will always be more apt to induce naturalists to examine more closely the variety in question, than the admitted or supposed want of distinctness of some varieties of another species, which has already been correctly described.

Although HAÜY may have traced the connexion between his *Diallage verte* and some greenish-grey, and blackish varieties, yet a similar passage is entirely wanting among these, and the two subdivisions of his *Diallage métalloïde*, the natural-historical species of diatomous and hemiprismatic Schiller-spar. In the first of these, the Diatomous Schiller-spar, the regular forms are as yet unknown; but, besides the difference in the hardness of that substance and Green Diallage, it is impossible to join into one continuous series the degrees of their specific gravity. As to the second, or Hemiprismatic Schiller-spar, that identity in the series of crystallisation, which is requisite to establish a transition, is entirely wanting, at least in so far as we are yet acquainted with the forms which it presents.

The first naturalist who considered Smaragdite as a species of its own, was the elder SAUSSURE, by whom it was first accurately described, and provided with the above-mentioned

* MOHS' Grund-Riss der Mineralogie, Th. i. s. 408.

name *. Modern mineralogy itself can scarcely be said to go beyond that period. *Smaragdite* was joined to *Felspar* by ROMÉ DE L'ISLE, and to *Schörl* by DE BORN. On account of the difference in the perfection of its laminæ, HAÜY gave it the name of *Diallage* †, and thus introduced it into the mineralogical systems. When HAÜY was publishing the first edition of his *Traité*, he was not yet acquainted with those substances which in the second he calls *Diallage métalloïde;* but he exhibits among his Diallage, under the very same denomination, the varieties of *Hypersthene* (prismatoidal Schiller-spar), which he afterwards established as a particular species. WERNER ‡ does not acknowledge Diallage as a particular species, but considers it as a variety of granular Actinolite. Very little, at that period, had yet been prepared for a more accurate examination of the natural properties of minerals, and in determining new varieties, any principle upon which it was possible to rely, was almost entirely wanting. Destitute of the means now afforded to the mineralogist, WERNER has, in the present instance, displayed that remarkable acuteness, by which he often discovered the close natural-historical relation of certain minerals of a very different aspect, whilst, on the other hand, he pointed out a difference between substances, very much resembling each other, which it required a long period of time to demonstrate by examining their properties with the greatest accuracy. But even WERNER has often been deceived, by founding his determinations upon mere ocular inspection; and we shall be the more disposed to abandon so deceitful a method of observation, the more we are able to ascertain those properties of mi-

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* Voyages dans les Alpes, t. v. § 1313.

† Traité, t. iii. p. 125.

‡ HOFFMANN, "Handbuch der Mineralogie," continued by BREITHAUPT, Th. ii. 2. s. 300.

nerals, which are capable of being expressed by numbers, for they must uniformly appear the same, to all who examine them with the necessary accuracy. Werner's collection at Freyberg, contains as granular Actinolite the varieties from Corsica, those from the valley of Saass, at the foot of Monte Rosa, and those from the Bacher in Stiria. It is to the last of these that the epithet of "granular" more particularly refers. Karsten * assumes the varieties from Corsica as a particular species he calls Schmaragdite, which, however, he mentions as including only a part of the Smaragdite of Saussure, the other belonging to chatoyant Hornblende (diatomous Schiller-spar). He was thus forced to acknowledge the high degree of resemblance among the less distinct varieties of Smaragdite and common Hornblende. In the same manner Steffens † enumerates in his system a particular species, of which the Smaragdite of Saussure forms the most distinct variety; yet this author confesses himself uncertain with respect to an exact natural classification, in regard to that species, and to those of the genus Schiller-spar in general, and therefore gives his determination merely as a temporary one, till further researches should decide its correctness. An opinion widely different from the preceding is entertained by Hausmann. His system ‡ joins in the substance Heterotype, the formations of Hornblende (hemiprismatic Augite-spar),—Diallage,—Bronzite (hemiprismatic Schiller-spar),—Hypersthene (prismatoidal Schiller-spar),—Anthophyllite (prismatic Schiller-spar),—together with several others that belong to the natural-historical species of hemiprismatic Augite-spar. As farther subdivisions, Diallage contains the follow-

* Tabellen, 1800, p. 70.; 1808, p. 40.

† Handbuch, Th. i. s. 326.

‡ Handbuch, Th. ii. p. 712.

ing kinds, viz. 1*st*, The real *Smaragdite*, of a grass-green colour; 2*d*, The less distinct varieties of the same, as *common;* and, 3*d*, The diatomous Schiller-spar from the Hartz, as *talcous Diallage ;* part of the latter forms probably also the 4*th* kind, or *Schiller-stein.* HAUSMANN endeavours to explain the single or monotomous cleavage of the different species of Schiller-spar, and the splendent laminæ of Diallage, as produced out of the two faces that occur in Hornblende, by supposing one of them to become more perfect at the expence of the other, whereby, in most cases also, " the angle of inclination is changed, and approaches more or " less to a right angle." A supposition of that kind, if granted, would be sufficient to overthrow the whole of crystallography, and that which has just now been quoted, has, for this reason, been very strongly objected to by HAÜY *. The substances and formations, in fact, of HAUSMANN'S system, do not allow of any strict comparison with either genus and species, or with species and subspecies; hence it will be impossible to assert, whether or not HAUSMANN means to comprehend Smaragdite under the species of Hornblende.

This latter opinion, very often quoted, indeed, but more commonly with the view of refuting it, seems in general to have gradually disappeared; and the most recent systematical mineralogists agree in considering the perfectly foliated green varieties of the Smaragdite of SAUSSURE as a particular species, which they place accordingly in their systems. Yet among the vast number of works that more especially treat of the present subject, there is none which does not attempt to reproach the rest with the uncertainty of their determinations, and the want of distinctness in their descriptions, though its own assertions can

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* Journal des Mines, XXXVIII. p. 161.

only serve farther to increase the embarrassment, for it is not under such circumstances that a complete list of quotations, or a thorough knowledge of the authors consulted, can be of any use. We must, therefore, recur to Nature herself, and conform our opinions to her dictates.

The principal properties in which Diallage, in its most distinct varieties, is said to differ from other minerals, more or less resembling it, consist in the grass-green colour, and in the facility of being reduced by fracture into thin laminæ, of a bright pearly lustre. All the works hitherto published on the subject, state these to be produced by cleavage, and yet they are not owing to *cleavage*, but merely to *composition*.

It has been reserved to the most recent period, to ascertain the real and important difference between faces of cleavage and faces of composition, which, indeed, are sometimes hardly distinguishable, if the latter possess a regular disposition in the interior of massive or crystallised varieties that seem to consist of a single individual. As far as I know, Professor MOHS is the first author who succeeded in giving an accurate definition of the phenomenon of cleavage in crystallised bodies. He confined it to the property of a *simple* mineral, of allowing its particles to be separated in more or less regular faces, in various yet determined directions. Only this property, and not the faces themselves, exist before the cleavage had been rendered visible by the assistance of mechanical force *. It is quite another thing with those faces of composition, which preserve constant directions within regularly formed crystals of certain minerals. They really exist before the mechanical division of the particles has been effected. This can be proved by many examples, and, among others, with a peculiar degree of evidence, by one of the most common in na-

* MOHS' Grund-Riss. Th. i. p. 264., &c.

ture, taken from Calcareous-spar. Here the faces of composition are parallel to those of R—1, the rhombohedron truncating the terminal edges of R, or the fundamental form; they are always the consequence of thin laminæ, or films of the same substance, engaged in the mass in an opposite sense, whose exact directions we can geometrically construct, by supposing one part of the mass contained between two faces parallel with each other, and with one of the faces of R — 1, to be revolved 180° round a line perpendicular to these faces, considered as their axis of revolution. Not even the perfectly transparent varieties from Iceland are free from such laminæ; their action upon light has led Dr Brewster to ascertain their existence. If they occur in greater number parallel to only one of the faces of R—1, they give rise to a variety, in which only one of the faces of cleavage, parallel to R, is very deeply striated in the direction of its horizontal diagonal. The seeming difference in the angles, owing to that composition, has induced Bernhardi to consider these varieties as belonging to a particular species, which he calls Streifenspath, or Striated-spar. It is necessary, therefore, to be very careful, in ascertaining whether an observed face in the interior of a crystal or of a cleaveable mass, be really produced by cleavage, or whether it owe its existence to regular composition.

Certain faces of cleavage, not parallel to the primitive form, have been said by Haüy to pass alongside of the molecules, without touching or intersecting them, which faces, for that reason, he called *des joints surnuméraires*. This explanation applies remarkably well to the above mentioned faces of composition, which have been so accurately described by Haüy * in a variety of green Diallage, where their pearly lustre is peculiarly bright.

* Traité, 2de Ed. v. ii. p. 462.

But this variety, likewise, contains real faces of cleavage situated in other directions, which intersect the former in oblique angles. They are but slightly marked, and rather incohering, wherever the brightest green colour is joined to a more perfect appearance of the laminæ; whereas they assume a higher degree of distinctness in the darker blackish-green varieties, into which the former pass without in the least changing their parallel position, exactly as is mentioned by Haüy. There are two such faces of cleavage, intersecting the faces of composition at an angle of 152°, while they intersect each other at an angle of 124°. Accurate measurements, taken by means of Dr Wollaston's goniometer, have yielded the angle formed by the two faces of cleavage in hemi-prismatic Augite-spar = 124° 13′, very little different from 124° 15′, the result of Nordenskiöld's observations, both of these being different from 124° 34′, the angle indicated by Haüy. If, according to the crystallographic method of Professor Mohs, we designate this prism by $(P\breve{r}+\infty)^3$, which sign it receives by being considered in connection with the rest of the simple forms in the same species, the face of composition, parallel to the greater diagonal of this prism, or to the lesser one of $P+\infty$, will be expressed by the sign $P\breve{r}+\infty$.

The regular composition of the twin-crystals of the present species, is parallel to the very same face $P\breve{r}+\infty$, of which, among many well known examples, I shall only mention the varieties found in the basalt of Bohemia. Several varieties of common Hornblende present a similar composition, as, for instance, that contained in the zircon-syenite from Friedrichsvärn in Norway; but none of the varieties, not even those of green Diallage, exhibit these faces of composition so perfectly smooth and even, as the greenish-black Hornblende from the Kiennerud mine near Kongsberg in Norway; and yet this has never been considered as a variety of Diallage.

Haüy quotes among the peculiarities of certain *joints surnuméraires*, that they contain films of heterogeneous minerals, a property in which they likewise agree with the above-mentioned faces of composition. Thus the black Hornblende from the Kiennerud mine, contains laminæ of Talc (prismatic Talc-mica); many varieties of the green contain laminæ of paratomous Augite-spar.

If we try to determine the specific gravity of two varieties of Diallage of a different colour, the grass-green and those of a darker shade, which, at the same time, are found to occur in one identical specimen, we meet very often with a difference amounting to, or even exceeding, one-tenth. Thus, in one instance, where both the varieties were remarkably distinct, I found the specific gravity of the green, at a temperature of 15° centigr. (59° Fahr.) to be $=3.129$ (agreeing with Saussure's statement of 3.140), whilst that of the greenish-black was only $= 3.007$,—a difference, the cause of which I was naturally led to inquire into more closely. When examining the darker parts, and comparing them with the green, I found that in the latter there were thin laminæ of another mineral, interposed between the particles of hornblende, the face in which they meet being that of $P\breve{r}+\infty$. These laminæ are not at all cleavable in the direction of the faces of $(P\breve{r}+\infty)^3 = 124° 13'$, but they may be cleaved in other directions, intersecting the axis of that prism under oblique angles, the latter faces of cleavage being much inferior to the former, both in lustre and continuity. In several of the varieties of this mineral from the Bacher, the interposed laminæ have increased in thickness, so far that the hemiprismatic Augite-spar, entirely disappears from the composition, and we are led to a mineral, which, with all its peculiarities, except the colour of grass-green, we must acknowledge to be a variety of paratomous Augite-spar. Now, we find the specific gravity of this

$= 3.232$, and its cleavage, exactly like that of all other varieties of paratomous Augite-spar, to take place in the directions of $(P\breve{r} + \infty)^3 = 87^\circ\ 33'$ (according to Nordenskiöld, $= 87^\circ\ 42'$ according to Haüy), and in those of its diagonals $P\ddot{r} + \infty$ and $P\bar{r} + \infty$. There is a face of composition still conspicuous in these varieties, but this face is parallel to $-\frac{P\breve{r}}{2} = 73^\circ\ 54'$ (according to Haüy) in paratomous Augite-spar, as in the varieties called Salite, some of the Mussite and common Actinolite of Werner; whereas in the hemiprismatic, we have found it parallel to $P\breve{r} + \infty$. Yet there exist varieties of paratomous Augite-spar, which are likewise compound, parallel to the face of $P\breve{r} + \infty$; this is the case in several Mussites, common Actinolite, &c. The specific gravity of 3.350, mentioned by Breithaupt in Hoffmann's *Handbuch*, after the authority of Werner, for his granular actinolite, refers to granular varieties of paratomous Augite-spar, since it is comprehended between the limits of the specific gravity of paratomous, but not between those of hemiprismatic Augite-spar.

From these observations we may conclude, that green Diallage, even in the most distinct varieties, is not a species of its own, but that it consists of the varieties of two other species, of paratomous and hemiprismatic Augite-spar, into each of which it insensibly passes, when the relative proportion of the other by continual diminutions finally disappears. Yet a passage of this kind never can entitle us to consider Smaragdite as the link which would join the two species, and consequently prove them to be but a single one; it proves nothing else but that Smaragdite is a mixture of both. This mixture of the two species in the perfectly foliated varieties of Diallage has been excellently described by Haüy, in giving its geometrical character in the first edition of his *Traité*, where he mentions that the single laminæ are very

often cracked in other directions than those of the less distinct faces of cleavage, the latter of which correspond to $P\bar{r}+\infty$ of hemiprismatic Augite-spar, and that *they shew a tendency of separating into rhombs*,—a property owing to the real faces of the cleavage of paratomous Augite-spar, which is spread in thin laminæ throughout the mass of the hemiprismatic. As long as it is within the powers of our senses to distinguish, and to separate these heterogeneous substances, the application of the *Characteristic* will clearly lead to their respective species; and no doubt in regard to their determination can ever remain, as long as their natural-historical properties can be ascertained with a sufficient degree of exactness.

Professor Mohs, very long ago, and even during his residence at Gratz, in consequence of accurate investigations, had recognised the granular varieties from the Saualpe and the Bacher, as belonging to paratomous, the latter also in part to hemiprismatic Augite-spar, and united them accordingly with these species. The only varieties still doubtful were those from other countries, so very much distinguished by the perfection and high pearly lustre of their laminæ. The establishment of Smaragdite into the species of axotomous Schiller-spar in the *Characteristic* of Mohs, was almost entirely founded upon foreign authority. It disappears now from the series of particular mineral species, all its varieties having been referred to those of hemiprismatic and paratomous Augite-spar, in the properties of which they partake.

Although whatever belongs to the determination of the species has thus been completed, and therefore the more peculiar object of this paper attained; yet I shall conform to the usage hitherto received in mineralogy, not to pass over entirely the instruction derived from other sciences foreign to natural history, in respect to the species under consideration, and shall therefore give a brief account of the chemical constitution of

Smaragdite, of its geognostic position, and concomitant minerals. Fortunately the labours of some of the most celebrated chemists and geologists afford the means of doing so.

The following analyses of the Smaragdite have been published for some years.

	1. Green Smaragdite, according to Lelièvre.	2. Green Smaragdite, according to Vauquelin.	3. Grey Smaragdite, according to Lelièvre.	4. Granular Actinolite from Stiria, according to Klaproth.
Silica, - - -	51.0	50.0	50.0	56.0
Alumina, - -	13.5	11.0	7.0	3.25
Magnesia, - -	5.0	6.0	8.0	18.5
Lime, - - -	14.5	13.0	17.0	15.5
Oxide of Iron,	8.0	5.3	14.5	4.75
Oxide of Copper,	0.5	1.5	0.0	A trace of Oxide of Manganese.
Oxide of Chrome,	4.0	7.5	0.0	1.0
	96.5	94.3	96.5	99.0

The agreement between the three first of these analyses, and the results obtained by analyzing the several varieties of Hornblende, is as obvious as the coincidence of that by Klaproth with the components of certain varieties of Augite, although, in general, these two species themselves do not present any striking difference in the proportions of their constituent parts.

The geognostic relations of what has been called Smaragdite and Diallage, seem to present more peculiarity. It is principally to Von Buch that we are indebted for having ascertained its geognostic position, and a great number of its localities, of which he has published the accounts in several of his works, and especially in two papers on *Gabbro*, inserted in the Memoirs of the Berlin Society. Throughout these papers he

has adopted Haüy's opinion, that Green Diallage is different from either Hornblende or Augite, and that it constitutes but one and the same species with *Diallage métalloïde;* and, in many instances, even the determination of the rocks is founded upon this supposition.

The mineral most commonly accompanying green Diallage, is *Saussurite* or *Jade*, a species considered as a variety of compact Felspar, by both Werner * and Haüy †, although it had long before been pointed out as a particular species, and acknowledged as such by a great number of mineralogists. Haüy takes for granted that the forms are identical, but even these deviate in Saussurite from the forms of the different species of Felspar: for it is possible to obtain its forms of cleavage, out of certain granular varieties, in the shape of oblique-angular four-sided prisms, of nearly 124° and 56°, which can very easily be cleaved, parallel to their faces, and with a less degree of distinctness, also parallel to their short diagonal. The difference between the specific gravity of Saussurite, and that of the Felspars, is as remarkable, and still more easy to be ascertained; the limits of the former being 3.2 and 3.4, whereas the latter never exceeds 2.8. The specific gravity of a compact Saussurite from Corsica, I found $=3.206$; that of two specimens of the granular variety, one from Piedmont, and another from Bayreuth, $=3.253$; of a compact one from the shores of the Lake of Geneva, $=3.343$. The hardness of Saussurite, is by many authors said to be superior to that of rhombohedral Quartz; yet it is no higher than 5.5 of the scale of Mohs, (between Apatite and Felspar), if taken with the necessary precautions by the assistance of a file. On account of its remarkable toughness, it nevertheless very often scratches rhom-

* Hoffmann's Handbuch, Th. ii. a. s. 339.

† Traité, 2de Ed. T. iii. p. 95.

bohedral Quartz, and emits lively sparks, if struck with steel. Of the rest of the minerals generally occurring along with Diallage, the most frequent are; *Garnet*, of a reddish colour, inclining to grey, lustre resinous, hardness = 7.5, specific gravity = 3.647; likewise *Talc* and *Kyanite*. These species, if intermixed with each other, constitute that rock which has been called Gabbro by Von Buch; but the relative quantities of the ingredients not being every where the same, I shall shortly mention those in some of the most generally known varieties.

The beautiful *Verde di Corsica duro*, almost entirely consists of compact Saussurite, of different shades of bluish-grey, in clouded delineations, containing small imbedded masses of green Smaragdite, which is very nearly pure hemiprismatic Augite-spar. Its specific gravity is = 3.000; the face of composition is not very distinct, and rather incohering, while the real cleavage, parallel to the faces of the prism, is more distinct, from the co-existence of which and the former the silky lustre is derived, particularly if its varieties be cut and polished. It contains, moreover, small masses of Talc, with delicate acicular crystals of that variety of hemiprismatic Augite-spar, which has received the name of Actinolite.

The Saussurite, forming the paste of the Gabbro, from the Valley of Saass, is greenish-grey, and nearly compact; the Smaragdite which it includes, sometimes in masses of considerable dimensions, is mountain-green; it presents very little lustre upon the faces of composition, and almost no traces of cleavage. Yet these varieties of hemiprismatic Augite-spar, seem to contain but little heterogeneous matter, their specific gravity being = 3.056. The faces of cleavage, appear more distinctly, in certain veins of perfectly pure varieties of the same species, which traverse in all directions the larger masses of Smaragdite, with which, however, they always remain in a parallel position. In this variety of Gabbro, we likewise meet with particles of Garnet and Talc, con-

taining crystals of Kyanite. Several other varieties from the same locality, and from Piedmont, are composed of a coarser grained greyish-white Saussurite; they contain a greater proportion of Garnet; their Smaragdite is accompanied and intermixed with Augite (paratomous Augite-spar), gradually increasing, so that the laminæ, in many instances, entirely pass into the latter species. Sometimes it is accompanied by a dark-green granular mixture of Saussurite and hemiprismatic Augite-spar.

The gabbro from Bayreuth consists of greyish-white Saussurite, and paratomous Augite-spar, of nearly the same variations of colour, slightly inclining to green. The specific gravity of the latter is = 3.255.

The varieties from the Bacher mountain, in Lower Stiria, are among the most remarkable. In comparison with those described above, they contain only a small quantity of Saussurite, but a great deal of Garnet, and also Kyanite, in beautiful blue crystals and grains. The large foliated masses of Smaragdite are seldom to be met with among them; more commonly their constituent parts separate into granular aggregates of the two species, paratomous and hemiprismatic Augite-spar, the colours of the former being more lively grass-green or greenish-grey, whilst those of the latter incline to brown, since they are, in general, either leek- or pistachio-green. They also differ in point of transparency, the hemiprismatic presenting a higher degree than the paratomous Augite-spar. The most instructive specimens in this respect can be gathered from the stone heaps prepared for the improvement of the road between Windisch-Feistritz and Gonnewitz, in Lower Stiria.

I have not found an opportunity of examining the composition of the Norway varieties of Gabbro. To Dr Naumann of Jena, I am indebted for several very distinct specimens of the Diallage contained in the varieties from Vaage and Gulfield. They are pure hemiprismatic Augite-spar, perfectly cleavable pa-

rallel to the prism $(P\breve{r}+\infty)^3 = 124° 13'$; and although compound, in the direction of the faces of $P\breve{r}+\infty$, they nevertheless present but a very inferior degree of lustre, upon these faces of composition, if compared with those of cleavage. The colour is a dark greenish-grey, and the specific gravity = 3.043.

Besides the rocks described, there are still a great many whose characteristic ingredient is said to be Diallage, which, however, do not contain any hemiprismatic Augite-spar at all, but only the varieties of the paratomous. Thus, the *Eclogite* of Haüy, found in the Saualpe in Carinthia, is a mixture of red Garnet and leek-green, paratomous Augite-spar, of the variety called Omphazite by Werner. The rocks from Austria and Bayreuth, which contain Omphazite, though smaller grained, are exactly of the same composition. The variety from the Saualpe occasionally contains small greenish-white prisms of *Zoisite* (prismatoidal Augite-spar).

Von Buch has sufficiently ascertained the remarkable geognostic relation between Gabbro and Serpentine, in many places which he visited during his travels. The Bacher in Lower Stiria, gives another proof of this fact. Here the Gabbro rock appears to constitute considerable masses in the Serpentine, nearly in the same way in which the masses of ore usually occur in beds, or still more distinctly in metalliferous veins, if we may indulge ourselves in considering the latter as points of comparison. Yet it has not been possible to strengthen this opinion, by observing the immediate contact of the two rocks; it is rather the consequence of their distribution over the surface of the country.

It is attended with less difficulty to determine the position of the rock from the Saualpe. It constitutes a subordinate bed in the gneiss, and mica-slate formation of the Alps, and has been opened by a quarry situate at the southern extremity of the Saualpe, in

the neighbourhood of a spring called the Kupplerbrunn, from which quarry the country around is supplied with a coarse kind of very hard mill-stones. The rock contains in this quarry thin layers of Quartz, filled with a great number of beautiful varieties of the Carinthine of WERNER (hemiprismatic Augite-spar), of Kyanite, of Zoisite, of crystallised varieties of Rutile (peritomous Titanium-ore), and of many other minerals we admire in collections. The same rock is also met with in the north part of the same mountain; and Professor MOHS has found it in his travels through the Stirian and Carinthian mountains, to extend to the Koralpe, and the more western parts of the Bacher, the east end of which is occupied by the Gabbro described above.

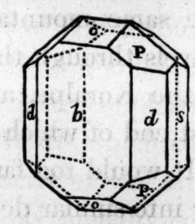
It would too far exceed the limits of the present paper to enter into similar details upon another class of rocks, likewise called Gabbro, or Euphotide, the chief ingredients of which are, not the green, but the metalloidal Diallage (hemiprismatic Schiller-spar), Labrador-felspar, and partly also Serpentine; as, for instance, in that from the Monte Ferrato, near Florence *. I only venture to add a few remarks as to the last of these species.

Serpentine has been pronounced by VON BUCH to be nothing else but a fine-grained mixture of *Gabbro* and *Talc*,—an opinion rather too much the result of mere speculation, than should ever be the case in a science connected with nature; yet the doubts respecting the existence of a particular species of Serpentine, which he derives from this opinion, are strongly supported

* According to the descriptions given by Drs HIBBERT and MACCULLOCH, the Diallage-rock from Shetland belongs likewise to this class. I cannot pass entirely unnoticed their labours in ascertaining the geological positions of these rocks; but not yet having had an opportunity of examining the rocks themselves, and having only recently become acquainted with the accounts published by Dr HIBBERT in his Description of the Shetland Islands, p. 372., and by Dr MACCULLOCH, in the Quarterly Journal of Science, Literature and the Arts, vol. x. p. 103., I cannot at present dwell any longer upon this subject.

by the highest authorities. Among all the proofs for the peculiarity of a species, there is none more evident than that founded upon the existence of crystalline varieties. It seems that the rarity of crystals of Serpentine, has been the reason of their not sooner having been noticed by mineralogists; although, perhaps, they have long ago been observed, but not sufficiently distinguished from other substances.

The forms of Serpentine are combinations of the Prismatic System of Mohs. One of its most common varieties is that represented in the annexed figure. According to the method of designation of Professor Mohs, its crystallographic sign is,



$\mathrm{P\breve{r}}$.	P.	$(\mathrm{P\breve{r}} + \infty)^3$.	$\mathrm{P\breve{r}} + \infty$.	$\mathrm{P\bar{r}} + \infty$.
o	*P*	*d*	*b*	*s*

The measures of the angles of P, the fundamental pyramid, are: 139° 34′, in the place of its more obtuse terminal edges; 105° 26′ in that of the more acute ones, replaced in the represented variety by the planes *o* of the horizontal prism $\mathrm{P\breve{r}}$; 88° 26′ at the basis. The ratio of the axis, and the diagonals $a : b : c$ is $= 1 : \sqrt{4.3} : \sqrt{1.4}$, which, however, must not be taken for any thing more than an approximation, the dulness of the faces being prejudicial to the employment of the reflecting goniometer. The angles of the prisms are the following: $\mathrm{P\breve{r}} = 128° 31'$, the angle contiguous to the terminal edge; $(\mathrm{P\breve{r}} + \infty)^3 = 97° 33'$ in the place of *b*, $= 82° 27'$ in that of *s*, the faces *b* and *s* forming truncations upon the respective obtuse and acute lateral edges of the prism *d*. Several of these crystals have one-fourth of an inch, or even more, in their greatest extension: they adhere to the sides of small cavities in the interior of a massive Serpentine, which moreover contains particles of Calcareous-spar.

I could get no information respecting their locality; but there exists a variety of the same species in roughly terminated imbedded crystals in the Weiss-stein of Chursdorf, near Penig in Saxony, where they occur in small irregular veins, composed of Felspar, Quartz and Tourmaline. The crystals of Serpentine are not pseudomorphous; for, besides the peculiarity of their series of crystallisation, they present very distinctly, faces of cleavage in the direction of the prism $(P\breve{r} + \infty)^3 = 82° 27'$, and of $P\breve{r} + \infty$, its greater diagonal, exactly agreeing with their external form. The imbedded crystals, too, distinctly exhibit these faces of cleavage, even if the external form should be entirely obliterated by their contact with other minerals.

Yet an exact knowledge of the forms was not required; and even without it, the specific gravity, if properly attended to, would have been an argument not to be refuted, against the opinion that Serpentine is identical with a mixture of Gabbro and Talc. The component parts of Gabbro are the following species:

Saussurite, the specific gravity of which is -	above	3.2
Hemiprismatic Augite-spar, - -	above	2.9
Paratomous Augite-spar, - -	above	3.2
Labrador, - - -	above	2.69
Talc, - - - -	above	2.7 .

Now, the specific gravity of Serpentine being $= 2.507$, in the above-mentioned crystallised variety, and $= 2.488$ in the cleavable one from Saxony; it appears not only below a mean term of all the specific gravities of the above-mentioned minerals, but far less than the lowest limit of any one of those species taken separately, which are said to produce the mixture.

Yet there exist some varieties of Serpentine, which, to a somewhat higher degree of specific gravity, join distinct traces of a crystalline formation. Thus the variety from Monteferrato gives 2.716; it contains no magnetic (octahedral) Iron-ore, as it does not affect the magnetic needle, which is the case with other va-

rieties that shew a higher specific gravity, namely, the Serpentine from Matrey in the Tyrol, G. = 2.700, from the Hartz, G. = 2.684, from Lower Stiria, G. = 2.667. The difference of the latter can easily be explained by an admixture of Iron-ore, for 8 per cent. of this mineral mixed with 92 per cent. of a Serpentine, whose specific gravity is = 2.5, will suffice for raising that of the compound to 2.72; but the same explanation cannot apply to the Serpentine from Monteferrato, and a difference of that kind, not yet accounted for, must, of itself, be sufficient for directing the attention of mineralogists towards a more accurate examination in future, of the different varieties of Serpentine.

Thus, mineralogy being the natural history of the mineral kingdom, must correctly determine the different varieties, and collect them within well defined species; but in this, it has fulfilled its duty, at least as far as respects the establishment of the species, and it must leave a great deal of valuable information to be obtained from other sciences, which likewise refer to the mineral productions, as, for instance, from natural philosophy, geology, chemistry, the art of working and smelting ores, &c. The first determination of a mineral, if incorrect, is very often prejudicial to all the subsequent inquiries, and it would not be difficult, from more than one example, to support this observation. An accurate investigation of the rocks, can alone enable the geologist to ascertain the nature of the materials of which mountains are composed, the grand object of his researches; and hence the fundamental knowledge of the mineral species which natural history affords, is as necessary to him, as it is to the chemist, who intends to subject them to his analysis.

G. Rose has given us a beautiful example of the latter, in his memoir on the different substances hitherto indiscriminately comprised under the name of Felspar *. The difference among

* Gilbert, Annalen der Physik, &c. 1823.

these minerals is so obvious, that it will be easily found in nature, even by those mineralogists, who have not been aware of it, previous to the publication of that paper; and no doubt but the resolution of what has been taken for only one species into several, must prove of great influence upon the former, namely, the determination of the rocks. In this respect, besides its pure natural-historical purpose, the above examination of Smaragdite will, I believe, not be found entirely devoid of interest.

X. *Investigation of Formulæ, for finding the Logarithms of Trigonometrical Quantities from one another.* By WILLIAM WALLACE, F. R. S. Edin. and Professor of Mathematics in the University of Edinburgh.

(Read November 3. 1823.)

THE most simple and obvious use of the Trigonometrical Tables, is to find the logarithmic sine, tangent, &c. corresponding to a given angle; and, reversely, the angle corresponding to a given sine or tangent. However, in their more general application, we have often to find a logarithmic cosine, having given the corresponding sine, or the contrary; also the sine or cosine from a given tangent, and, in these cases, it may be of no consequence to know the exact angle.

Supposing a logarithmic cosine to be given, to find the sine, it is usual, first, to find the angle from the cosine, and then the sine from the angle *. In this way, as the given cosine may not be found exactly in the Table, the differences must be taken, and two proportions made to find a correction, to be added to the tabular sine next less to that which is required. Indeed the two proportions may be brought into one, by leaving out the ratio of the angles, which is common to both, and introduced unnecessarily; but, in either way, the result is an approximation, only of the first degree, near enough, indeed, for most purposes, but which, in certain cases, may not be sufficiently correct.

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* CAGNOLI Trigonometrie, Art. 428. 2de Edition.

For example, let the sine be required, corresponding to the cosine 9.9450915962. Using VLACQ's *Arithmetica Logarithmica*, it appears that the given number is between the cosines of 28° 12′ and 28° 13′. The differences between the cosines and between the sines of these angles are now to be taken, and also the difference between the given cosine and the next greater in the Table, as follows;

cos 28° 12′	9.9451254712	sin 9.6744484704	cos 28° 12′	9.9451254712
cos 28 13	9.9450577094	sin 9.6746839948	given cos	9.9450915962
Differences,	677618	2355244		338750

and this proportion stated:

677618 : 2355244 : : 338750 : *correction*.

The fourth term, or *correction*, comes out 1177417, which, added to the sine of 28° 12′, gives 9.6745662121 for the sine required. This, however, is only accurate in the first seven figures, the true value being 9.6745662532, and the error .0000000411. We have therefore lost the advantage which may be derived from the logarithms in these Tables being carried to ten figures instead of seven, the common number.

Having been led to this subject, by what I consider an improvement in the mode of resolving a case in Plane Trigonometry, which will form the subject of a separate paper, I have investigated rules for deducing the logarithms of Trigonometrical functions from one another. The formulæ are indeed only approximations, but they are of the second degree, and therefore sufficiently accurate; and from their nature, they are well adapted to logarithmic calculation. As they appear to me to be new, and to possess considerable analytic elegance by their simplicity and compactness, I have ventured to lay them before the Royal Society.

1. Let Δx be the finite increment of any angle x, (the letter Δ being employed as the characteristic of a finite difference) and let $m = .4342945$, the modulus of the common system of logarithms. By TAYLOR's theorem,

$$\log\cos(x+\Delta x) = \log\cos x - m\left\{\tan x\cdot\Delta x + \frac{1}{\cos^2 x}\,\frac{(\Delta x)^2}{2} + \frac{\sin x}{\cos^3 x}\,\frac{(\Delta x)^3}{3} + \&c.\right\},$$

$$\log\sin(x+\Delta x) = \log\sin x + m\left\{\cot x\cdot\Delta x - \frac{1}{\sin^2 x}\,\frac{(\Delta x)^2}{2} + \frac{\cos x}{\sin^3 x}\,\frac{(\Delta x)^3}{3} - \&c.\right\},$$

Employing now the notation of the theory of differences, and expressing,

$$\log\cos(x+\Delta x) - \log\cos x \text{ by } \Delta\log\cos x,$$
$$\log\sin(x+\Delta x) - \log\sin x \text{ by } \Delta\log\sin x;$$

we get these two formulæ,

$$\Delta\log\cos x = -m\left\{\tan x\cdot\Delta x + \frac{1}{\cos^2 x}\,\frac{(\Delta x)^2}{2} + \frac{\sin x}{\cos^3 x}\,\frac{(\Delta x)^3}{3} + \&c.\right\}; \quad (1)$$

$$\Delta\log\sin x = m\left\{\cot x\cdot\Delta x - \frac{1}{\sin^2 x}\,\frac{(\Delta x)^2}{2} + \frac{\cos x}{\sin^3 x}\,\frac{(\Delta x)^3}{3} - \&c.\right\}. \quad (2),$$

Again, by TAYLOR's theorem, we have

$$\tan(x+\Delta x) = \tan x + \frac{1}{\cos^2 x}\,\Delta x + \frac{\sin x}{\cos^3 x}\,(\Delta x)^2 + \&c.$$

$$\cot(x+\Delta x) = \cot x - \frac{1}{\sin^2 x}\,\Delta x + \frac{\cos x}{\sin^3 x}\,(\Delta x)^2 - \&c.$$

and hence

$$\left\{\tan(x+\Delta x) + \tan x\right\}\frac{m\cdot\Delta x}{2} = m\left\{\tan x\cdot\Delta x + \frac{1}{\cos^2 x}\,\frac{(\Delta x)^2}{2} + \frac{\sin x}{\cos^3 x}\,\frac{(\Delta x)^3}{2} + \&c.\right\};$$

$$\left\{\cot(x+\Delta x) + \cot x\right\}\frac{m\cdot\Delta x}{2} = m\left\{\cot x\cdot\Delta x - \frac{1}{\sin^2 x}\,\frac{(\Delta x)^2}{2} + \frac{\cos x}{\sin^3 x}\,\frac{(\Delta x)^3}{2} - \&c.\right\}.$$

But by the calculus of sines,

$$\tan(x+\Delta x) + \tan x = \frac{\sin(2x+\Delta x)}{\cos x\cos(x+\Delta x)};$$

$$\cot(x+\Delta x) + \cot x = \frac{\sin(2x+\Delta x)}{\sin x\sin(x+\Delta x)};$$

Therefore,

$$\frac{\sin(2x+\Delta x)}{\cos x\cos(x+\Delta x)}\,\frac{m\cdot\Delta x}{2} = m\left\{\tan x\cdot\Delta x + \frac{1}{\cos^2 x}\,\frac{(\Delta x)^2}{2} + \frac{\sin x}{\cos^3 x}\,\frac{(\Delta x)^3}{2} + \&c.\right\}, \ (3)$$

$$\frac{\sin(2x+\Delta x)}{\sin x\sin(x+\Delta x)}\,\frac{m\cdot\Delta x}{2} = m\left\{\cot x\cdot\Delta x - \frac{1}{\sin^2 x}\,\frac{(\Delta x)^2}{2} + \frac{\sin x}{\cos^3 x}\,\frac{(\Delta x)^3}{2} - \&c.\right\}; \ (4)$$

By adding the corresponding sides of equations (1), (3), and subtracting those of (2), (4), we find

$$\Delta \log \cos x + \frac{\sin(2x + \Delta x)}{\cos x \cos(x + \Delta x)} \, \frac{m \cdot \Delta x}{2} = \frac{\sin x}{\cos^3 x} \, \frac{m(\Delta x)^3}{6} - \text{\&c.} \quad (5)$$

$$\Delta \log \sin x - \frac{\sin(2x + \Delta x)}{\sin x \sin(x + \Delta x)} \, \frac{m \cdot \Delta x}{2} = -\frac{\cos x}{\sin^3 x} \, \frac{m(\Delta x)^3}{9} + \text{\&c.} \quad (6)$$

And, again, by multiplying both sides of equation (5) by $\cos x \cos(x + \Delta x)$, and both sides of equation (6) by $\sin x \sin(x + \Delta x)$, and adding the results, there is obtained

$$\left.\begin{array}{r} \cos x \cos(x + \Delta x) \,.\, \Delta \log \cos x \\ + \sin x \sin(x + \Delta x) \cdot \Delta \log \sin x \end{array}\right\} = \left\{\begin{array}{l} \dfrac{\sin x \cos(x + \Delta x)}{\cos^2 x} \\ - \dfrac{\cos x \sin(x + \Delta x)}{\sin^2 x} \end{array}\right\} \frac{m(\Delta a)^3}{6} \quad \text{\&c.}$$

The expression which forms the coefficient of $\frac{m(\Delta x)^3}{6}$, is, by the calculus of sines, equivalent to

$$-\frac{\sin \Delta x + \cos 2x \sin(2x + \Delta x)}{2 \cos^2 x \sin^2 x};$$

But in the applications to be made of the formulæ, the angle Δx is always small; we may therefore reject the term $\sin \Delta x$, and assume $\sin(2x + \Delta x) = \sin 2x$; the expression will then become

$$-\frac{\cos 2x \sin 2x}{2 \cos^2 x \sin^2 x} = -\frac{\cos 2x \sin x \cos x}{\cos^2 x \sin^2 x} = -2 \cot 2x.$$

Thus we have

$$\left.\begin{array}{r} \cos x \cos(x + \Delta x) \cdot \Delta \log \cos x \\ + \sin x \sin(x + \Delta x) \cdot \Delta \log \sin x \end{array}\right\} = -\cot 2x \, \frac{m(\Delta x)^3}{3} \text{ nearly.} \quad (7)$$

The second member of this equation vanishes when x is half a right angle; and supposing the angle Δx to be one minute; when $x = 1^\circ$, or when $x = 89^\circ$, the function

$$-\cot 2x \, \frac{m(\Delta x)^3}{3} = \mp \cdot 0000000002,$$

the sign being $-$ in the former case, and $+$ in the latter; therefore in any Trigonometrical Tables hitherto constructed,

if x and $x + \Delta x$ be any two adjoining angles, we have in general

$$\cos x \cos(x + \Delta x) \cdot \Delta \log \cos x + \sin x \sin(x + \Delta x) \Delta \log \sin x = 0 \text{ nearly.}$$

And consequently

(A)

$$\sin x \sin(x + \Delta x) \cdot \Delta \log \sin x = \cos x \cos(x + \Delta x) (- \Delta \log \cos x) \, *.$$

This formula may be expressed independently of logarithms, by considering that

$$\Delta \log \sin x = \log \frac{\sin(x + \Delta x)}{\sin x}, \quad \Delta \log \cos x = \log \frac{\cos(x + \Delta x)}{\cos x};$$

Hence we have, nearly,

(A′)

$$\left\{ \frac{\sin(x + \Delta x)}{\sin x} \right\}^{\sin x \sin(x + \Delta x)} = \left\{ \frac{\cos(x + \Delta x)}{\cos x} \right\}^{-\cos x \cos(x + \Delta x)}.$$

2. The object I have in view being the determination of the logarithmic sine and cosine of an angle, the one from the other, to adapt the formula to that purpose, I express it in these two ways.

(B)

$$\Delta \log \sin x = \cot x \operatorname{cosec} x \cos(x + \Delta x) \frac{\sin x}{\sin(x + \Delta x)} (- \Delta \log \cos x);$$

(C)

$$- \Delta \log \cos x = \tan x \sec x \sin(x + \Delta x) \frac{\cos x}{\cos(x + \Delta x)} \cdot \Delta \log \sin x.$$

The same formulæ, expressed in logarithms, will stand ready for use thus:

(B′)

$$\text{Log}(\Delta \log \sin x) = \log\{\cot x \operatorname{cosec} x \cos(x + \Delta x)(- \Delta \log \cos x)\} - \Delta \log \sin x;$$

(C′)

$$\text{Log}(- \Delta \log \cos x) = \log\{\tan x \sec x \sin(x + \Delta x) \cdot \Delta \log \sin x\} + (- \Delta \log \cos x);$$

* The symbol $- \Delta \log \cos x$ here indicates, that the sign of $\log x - \log(x + \Delta x)$, (which is a negative quantity), is to be changed to +.

3. In applying the formulæ, let it first be supposed that a log. cosine is given, to find the corresponding sine: Then, putting $x + \Delta x$ for the angle corresponding to the given cosine, the next greater in the table will be $\cos x$; this, therefore, as well as $\Delta \log \cos x$ ($= \log \cos (x + \Delta x) - \log \cos x$), $\cot x$, $\operatorname{cosec} x$, and $\sin x$, will be given. Let us for a moment put

$$y = \Delta \log \sin x, \qquad p = \cot x \operatorname{cosec} x \cos (x + \Delta x) (-\Delta \log \cos x),$$

then formula (B′) will stand thus,

$$\text{Log}\, y = \log p - y;$$

here p is a known quantity, and we must find such a value of y as satisfies the equation.

Now, y is always small, because it is the logarithm of a quantity differing but little from an unit; therefore, as a first approximation,

$$\text{Log}\, y = \log p, \text{ and } y = p \text{ nearly};$$

and hence

$$\text{Log}\, y = \log p - p \text{ nearly}.$$

If the value of y, found from this last equation, be not sufficiently exact, it is at least a nearer approximation than the first assumption $y = p$; therefore, denoting it by y', we shall have still more correctly,

$$\text{Log}\, y = \log p - y'.$$

Thus, it appears, that to approximate to the value of y in the equation

$$\text{Log}\, y = \log p - p,$$

we have only to form a series of quantities y', y'', y''', &c. from the function p, such, that

$$y' = p, \qquad \log y'' = \log p - y', \qquad \log y''' = \log p - y'', \text{ \&c.}$$

and these will be successive approximations to the value of y.

Next, let us suppose that a log. sine is given to find the corresponding cosine; let $x + \Delta x$ be the angle to the given sine; the next less in the table will be $\log \sin x$, and their difference, (viz. $\Delta \log \sin x$) will be given, as well as $\tan x$, $\sec x$, and $\cos x$.

Let us now put

$$z = -\Delta \log \cos x, \qquad q = \tan x \sec x \sin (x + \Delta x) \cdot \Delta \log \sin x,$$

and instead of formula (C′) we have this:

$$\operatorname{Log} z = \log q + z.$$

To resolve this equation, we must form a series of approximations,

$$z' = q, \qquad \log z'' = \log q + z' \qquad \log z''' = \log q + z'', \text{ \&c.}$$

and the quantities z', z'', z''', &c. will quickly approximate to the correct value of z.

4. To make the mode of proceeding perfectly clear, I shall now give some examples.

(1.) To find the log. sine of an angle whose cosine = 9.9450803019 by VLACQ's *Arithmetica Logarithmica*, which contains a table of log. sines, &c. to every minute of the quadrant, and to ten places of figures:

Given cosine,	$\log \cos (x + \Delta x)$	$= 9{\cdot}9450803019$
Next greater in Tab.	$\log \cos x\ (28^\circ\ 12')$	$= 9{\cdot}9451254712$
	$-\Delta \log \cos x$	$= {\cdot}0000451693$

		Log.		
$\cot x$	-	$10{\cdot}2706770$		
$\operatorname{cosec} x$	-	$10{\cdot}3255515$		
$\cos (x + \Delta x)$	-	$9{\cdot}9450803$		
$-\Delta \log \cos x$	-	$\bar{5}{\cdot}6548434$		
$\log p$	-	$= \bar{4}{\cdot}1961522$	$y' = {\cdot}0001571\ldots$	First approx.
$\log p - y'$	-	$= \bar{4}{\cdot}1959951$	$y'' = {\cdot}0001570345 = \Delta \log \sin x$	
			$9{\cdot}6744484704 = \log \sin x$	
		The sine required,	$9{\cdot}6746055049 = \log \sin (x + \Delta x)$	

The sine required is that of 28° 12′ 40″, and its correct value, as given in VLACQ's *Trigonometria Artificialis*, is 9·6746055050, with which our approximation agrees almost exactly.

(2.) The log. cosine corresponding to log. sin 9·9983515861 is required?

Given sine, - -	$\log \sin (x + \Delta x)$	$= 9·9983515861$
Next less in Table,	$\log \sin x$ (85°)	$= 9·9983442260$
	$\Delta \log \sin x$	$= 0·0000073601$

	Log.			
$\tan x$ -	11·0580482			
$\sec x$ -	11·0597040			
$\sin (x + \Delta x)$ -	9·9983516			
$\Delta \log \sin x$ -	$\bar{6}·8668837$			
$\log q$ -	$= \bar{4}·9829875$	$z' =$	·0009616...First approx.	
$\log q + z'$ -	$\bar{4}·9839491$	$z'' =$	·0009637...Second.	
$\log q + z''$ -	$\bar{4}·9839512$	$z''' =$	·0009637210	$= - \Delta \log \cos x$
			8·9402960083	$= \log \cos x$
	The cosine required		8·9393322873	$= \log \cos (x + \Delta x)$

The cosine required is that of 85° 0′ 40″; its correct value is 8·9393322838, which differs from its value computed by our formulæ in the last two of the ten figures. A more correct result would have been obtained, if the given sine and the next less in the table had been continued to more decimal places, a small change in the sine of so large an angle as 85°, producing a considerable change in the cosine. This is an inconvenience in calculation which can only be obviated by more extensive tables.

(3.) Let it now be required to determine, by Briggs's Table (*Trigonometria Britannica*), the cosine corresponding to $\sin (x + \Delta x) = 9·99993516626353$, which is known to be the sine of 89° 0′ 36″.

Given sine,	$\log\sin(x+\Delta x)$	$= 9{\cdot}99993516626353$
Next less,	$\log\sin x\ (89^\circ)$	$= 9{\cdot}99993384980922$
	$\Delta\log\sin x =$	$\cdot 00000131645431$

	Log.		
$\tan x$	$11{\cdot}7580785313$		
$\sec x$	$11{\cdot}7581446816$		
$\sin(x+\Delta x)$	$9{\cdot}9999351663$		
$\Delta\log\sin x$	$\bar{6}{\cdot}1194057907$		
$\log q$	$\bar{3}{\cdot}6355641699$	$z' = \cdot 0043208000$...	First approx.
$\log q + z'$	$\bar{3}{\cdot}6398849699$	$z'' = \cdot 0043640000$...	Second.
$\log q + z''$	$\bar{3}{\cdot}6399281699$	$z''' = \cdot 0043644364$...	$= -\Delta\log\cos x$
		$8{\cdot}2418553184$...	$= \log\cos x$
The approx. value of cosine,		$8{\cdot}2374908820$	
Its true value is,	-	$8{\cdot}23749095$...	
Error of Approx.		$\cdot 00000007$	

From this example it appears, that, supposing the sine of an angle about 89°, or the cosine of an angle about 1° to be known with sufficient accuracy, the formula may be trusted to give the cosine or sine true to seven decimal places. Towards the middle of the quadrant its accuracy is much greater.

In these examples, the logarithmic differences to be found have been expressed by seven or more significant figures: it was therefore necessary in the calculation to take out the logarithms to at least as many places; but when the given sine or cosine consists of only seven figures, besides the index, the logarithms need not be carried so far; as in this example.

(4.) To find the sine corresponding to $\log\cos(x+\Delta x) = 9{\cdot}9409872$

In Hutton's Tables, next greater, $\log\cos x\,(60^\circ 49') = 9{\cdot}9410461$

$-\Delta\log\cos x \quad \cdot 0000589$

$\cot x$	-	$10{\cdot}25298$		
$\operatorname{cosec} x$	-	$10{\cdot}31193$		
$\cos(x+\Delta x)$	-	$9{\cdot}94098$		
$\log(-\Delta\log\cos x)$		$\bar{5}{\cdot}77012$		
$\log p$	-	$\bar{4}{\cdot}27601$	$y' = \cdot 0001888$...	First approx.
$\log p - y$	-	$\cdot 27582$	$y'' = \cdot 0001885$	$= \Delta\log\sin x$
			$9{\cdot}6880688$	$= \log\sin x$
Sine required,			$9{\cdot}6882573$	

The operations have been all put down at length; but in practice, the trouble of writing down so many cyphers, and the repetition of the same figures, may be spared, just as in the common operations of multiplication and division.

5. Before leaving the consideration of formula (B′), (C′), (art. 2), I may just observe, that they may otherwise be elegantly expressed thus:

$$\left\{\frac{\sin(x+\Delta x)}{\sin x}\right\}^{\frac{\sin(x+\Delta x)}{\sin x}} = \left\{\frac{\cos(x+\Delta x)}{\cos x}\right\}^{-\tan x \sec x \cos(x+\Delta x)} \;;\quad (\mathrm{B}'')$$

$$\left\{\frac{\cos(x+\Delta x)}{\cos x}\right\}^{-\frac{\cos(x+\Delta x)}{\cos x}} = \left\{\frac{\sin(x+\Delta x)}{\sin x}\right\}^{\cot x \operatorname{cosec} x \sin(x+\Delta x)} . \quad (\mathrm{C}'')$$

Here we see immediately, that the determination of the sine from the cosine, or the cosine from the sine, by the method we have followed, requires the resolution of the exponential equations,

$$y^y = p, \qquad z^z = q,$$

p and q being known, and y and z unknown quantities, and each having the value which it denotes in art. 3. The facility with which they have been found is the consequence of their being nearly $= 1$. The general solution of such an equation, however, is attended with more difficulty.

6. The determination of the sine from the cosine, or the cosine from the sine, enables us to determine the tangent from either the sine or cosine, (because $\tan x = \frac{\sin x}{\cos x}$) also the sine of twice the arc, which is equal $2 \sin x \cos x$, and hence, again, the cosine and tangent of double the arc.

7. I now proceed to investigate formulæ by which the logarithmic sine and cosine may be deduced from the tangent. By the calculus of sines,

$$\text{Log} \cos x = \log \tan x - \log \sin x,$$

therefore

$$-\Delta \log \cos x = \Delta \log \tan x - \Delta \log \sin x,$$

and hence, again,

$$\cos x \cos (x + \Delta x) \cdot (-\Delta \log \cos x) = \begin{cases} \cos x \cos (x + \Delta x) \cdot \Delta \log \tan x \\ - \cos x \cos (x + \Delta x) \cdot \Delta \log \sin x. \end{cases}$$

But from formula (A) art. 1. we have also

$$\cos x \cos (x + \Delta x) \cdot (-\Delta \log \cos x) = \sin x \sin (x + \Delta x) \cdot \Delta \log \sin x,$$

Hence, by putting the second members of these equations equal to one another, and transposing, we find,

$$\{\cos x \cos (x + \Delta x) + \sin x \sin (x + \Delta x)\} \cdot \Delta \log \sin x = \cos x \cos (x + \Delta x) \cdot \Delta \log \tan x.$$

The expression which forms the coefficient of $\Delta \log \sin x$ is by the calculus of sines equivalent to $\cos \Delta x$, therefore

$$\cos \Delta x \, (\Delta \log \sin x) = \cos x \cos (x + \Delta x) \cdot \Delta \log \tan x. \quad (8)$$

By a mode of proceeding in all respects similar to the above, we easily find

$$\cos \Delta x \, (-\Delta \log \cos x) = \sin x \sin (x + \Delta x) \cdot \Delta \log \tan x. \quad (9)$$

As the omission of the factor $\cos \Delta x$ will not affect the first two terms of the series, which express the values of $\Delta \log \sin x$, and $\Delta \log \cos x$, we may leave it out, and have more simply

(D)

$$\Delta \log \sin x = \cos x \cos (x + \Delta x) \; \Delta \log \tan x;$$

(E)

$$-\Delta \log \cos x = \sin x \sin (x + \Delta x) \; \Delta \log \tan x;$$

8. The degree of accuracy of these approximations may be estimated by comparing the series which express the exact values of $\Delta \log \sin x$, $\Delta \log \cos x$, and $\Delta \log \tan x$.

We have already found, art. 1, that

$$\Delta \log \sin x = m \left\{ \frac{\cos x}{\sin x} \cdot \Delta x - \frac{1}{\sin^2 x} \frac{(\Delta x)^2}{2} + \frac{\cos x}{\sin^3 x} \frac{(\Delta x)^3}{3} - \text{\&c.} \right\};$$

$$-\Delta \log \cos x = m \left\{ \frac{\sin x}{\cos x} \cdot \Delta x + \frac{1}{\cos^2 x} \frac{(\Delta x)^2}{2} + \frac{\sin x}{\cos^3 x} \frac{\Delta x^3}{3} + \text{\&c.} \right\};$$

Therefore, because $\Delta \log \tan x = \Delta \log \sin x - \Delta \log \cos x$,

$$\Delta \log \tan x = m \left\{ \frac{1}{\cos x \sin x} \cdot \Delta x - \frac{\cos^2 x - \sin^2 x}{\cos x^2 \sin^2 x} \frac{(\Delta x)^2}{2} + \frac{\cos^4 x + \sin^4 x}{\cos^3 x \sin^3 x} \frac{(\Delta x)^3}{3} - \&c. \right\}.$$

This last series being multiplied by

$$\cos x \cos (x + \Delta x) = \cos^2 x - \cos x \sin x \cdot \Delta x - \cos^2 x \frac{(\Delta x)^2}{1 \cdot 2} + \&c.$$

and the same series also by

$$\sin x \sin (x + \Delta x) = \sin^2 x + \cos x \sin x \cdot \Delta x - \sin^2 x \frac{(\Delta x)^2}{1 . 2} + \&c.$$

the results, continued as far as three terms, will be

$$\cos x \cos (x + \Delta x) \Delta \log \tan x = m \left\{ \frac{\cos x}{\sin x} \cdot \Delta x - \frac{1}{\sin^2} \frac{(\Delta x)^2}{2} + \frac{2 \cos^4 x - \sin^4 x}{\cos x \sin^3 x} \frac{(\Delta x)^3}{6} + \&c. \right\};$$

$$\sin x \sin (x + \Delta x) \Delta \log \tan x = m \left\{ \frac{\sin x}{\cos x} \cdot \Delta x + \frac{1}{\cos^2 x} \frac{(\Delta x)^2}{2} + \frac{2 \sin^4 x - \cos^4 x}{\cos^3 x \sin x} \frac{(\Delta x)^3}{6} + \&c. \right\}.$$

By comparing these series with the series for $\Delta \log \sin x$, and $-\Delta \log \cos x$, it will appear that

$$\Delta \log \sin x = \cos x \cos (x + \Delta x) \Delta \log \tan x + m\,(2 \cot x + \tan x) \frac{(\Delta x)^3}{6}, \ \&c. \quad (10)$$

$$-\Delta \log \cos x = \sin x \sin (x + \Delta x) \Delta \log \tan x + m\,(2 \tan x + \cot x) \frac{(\Delta x)^3}{6}, \ \&c. \quad (11)$$

Hence we may infer the following properties of formulæ (D) (E):

(1.) The two formulæ are least correct towards the extremities of the quadrant.

(2.) The first is most correct when $\tan x = \sqrt{\tfrac{3}{4}}$, that is, when $x = 54\tfrac{3}{4}^\circ$ nearly, and the second when $\tan x = \sqrt{\tfrac{1}{2}}$, in which case $x = 35\tfrac{1}{4}^\circ$ nearly, because, in these cases, the functions $2 \cot x + \tan x$, and $2 \tan x + \cot x$ are the least possible.

(3.) When x is a small angle, the error of the expression for $\Delta \log \sin x$ is nearly double that for $\Delta \log \cos x$. The reverse is true when x is nearly a right angle.

9. Supposing the increment Δx to be one minute, the error of the formula for $\Delta \log \sin x$, when $x = 2^\circ$, will be

.0000000001. Hence we may infer, that with a table of logarithmic sines and tangents to every minute, and to ten decimal places, such as that in Vlacq's *Arithmetica Logarithmica*, the log sine and cosine may be found from any log tangent correct to at least 10 decimals between $x = 2°$ and $x = 88°$. Beyond these limits the accuracy will diminish, but still they will be true to seven figures, when $x = 10'$, and when $x = 89° 50'$. With Vlacq's *Trigonometria Artificialis*, which gives the sines and tangents to every ten seconds and to ten figures, the formulæ will be accurate throughout the whole table.

10. The formulæ (D), (E) when properly adapted to calculation, will stand thus,

(D′)

$$\Delta \log \sin x = \sin x \cos x \cot (x + \Delta x) \frac{\sin (x + \Delta x)}{\sin x} \cdot \Delta \log \tan x ;$$

(E′)

$$- \Delta \log \cos x = \sin x \cos x \tan (x + \Delta x) \frac{\cos (x + \Delta x)}{\cos x} \cdot \Delta \log \tan x ;$$

And in logarithms

(D″)

$$\log (\Delta \log \sin x) = \log \{\sin x \cos x \cot (x + \Delta x) \cdot \Delta \log \tan x\} + \Delta \log \sin x,$$

(E″)

$$\log (- \Delta \log \cos x) = \log \{\sin x \cos x \tan (x + \Delta x) \cdot \Delta \log \tan x\} - (- \Delta \log \cos x).$$

11. The mode of deducing $\Delta \log \sin x$ and $\Delta \log \cos x$ from these formulæ, is exactly the same as we have already employed in art. 3. That is, putting

$$y = \Delta \log \sin x, \qquad z = - \Delta \log \cos x,$$

$$r = \frac{\sin x \cos x}{\tan (x + \Delta x)}, \qquad s = \sin x \cos x \tan (x + \Delta x),$$

if we find $y' = r$, $\log y'' = \log r + y'$, $\log y''' = \log r + y''$, &c.

Then y', y'', y''', &c. will be a series of successive approximations to y or $\Delta \log \sin x$.

Also if $z' = s$, $\log z'' = \log s - z'$, $\log z''' = \log s - z''$, &c. then shall z', z'', z''', &c. be a series of approximations to the value of z.

12. Our formulæ, although only approximations, are yet more accurate than is necessary with the ordinary tables. Others somewhat more simple, and sufficiently correct, may be deduced from them, as follows :

The two formulæ (D) (E) may evidently be written thus,

$$\Delta \log \sin x = \cos^2 x \, \frac{\cos (x + \Delta x)}{\cos x} \, \Delta \log \tan x;$$

$$-\Delta \log \cos x = \sin^2 x \, \frac{\sin (x + \Delta x)}{\sin x} \, \Delta \log \tan x.$$

Near the beginning of the quadrant, $\dfrac{\cos (x + \Delta x)}{\cos x} = 1$ nearly, and towards the end $\dfrac{\sin (x + \Delta x)}{\sin x} = 1$ nearly. Therefore, in these cases,

$$\left.\begin{aligned} \Delta \log \tan x &= \cos^2 x \, \Delta \log \tan x \text{ nearly;} \\ -\Delta \log \cos x &= \sin x^2 \, \Delta \log \tan x \text{ nearly.} \end{aligned}\right\} \quad \text{(F)}$$

13. That we may estimate the degree of approximation, let us multiply the series for $\Delta \log \tan x$ by $\cos^2 x$, and by $\sin^2 x$; the results are,

$$\cos^2 x \, \Delta \log \tan x = m \left\{ \frac{\cos x}{\sin x} \Delta x - \frac{\cos^2 x - \sin^2 x}{\sin^2 x} \frac{(\Delta x)^2}{2} + \text{\&c.} \right\}$$

$$\sin^2 x \, \Delta \log \tan x = m \left\{ \frac{\sin x}{\cos x} \Delta x - \frac{\cos^2 x - \sin^2 x}{\cos^2 x} \frac{(\Delta x)^2}{2} + \text{\&c.} \right\}$$

By comparing these formulæ with the series for $\Delta \log \sin x$, and $\Delta \log \cos x$ (art. 1.) we shall find

$$\Delta \log \sin x = \cos^2 x \, \Delta \log \tan x - m (\Delta x)^2 + \&c.$$
$$-\Delta \log \cos x = \sin^2 x \, \Delta \log \tan x + m (\Delta x)^2 + \&c.$$

Hence it appears, that our two formulæ F are only approximations of the first order; but that the error, as far as it depends on the square of Δx, is independent of the angle x, or is a constant quantity for a given value of Δx.

If we make the increment Δx an angle of one minute, we have $m (\Delta x)^2 = .0000000367$. Thus they appear to be correct in the first seven decimal places; we may therefore safely employ them with HUTTON'S or SHERWIN'S Tables, reserving the others for Tables of greater extent.

14. Let it be required to find the logarithmic sine and cosine corresponding to the tangent 10·076340410548.

The given tan $(x + \Delta x) = 10{\cdot}076340410548$
The next less (BRIGGS'S Trig. Brit.) tan x (50°) $= 10{\cdot}076186469801$

$\Delta \log \tan x = {\cdot}000153940747$

To find $\log \sin (x + \Delta x)$

$\sin x$	9·8842539666			
$\cos x$	9·8080674968			
$\cot (x + \Delta x)$	9·9236595895			
$\Delta \log \tan x$	$\bar{4}$·1873535895			
$\log r =$	$\bar{5}$·8033346424	$z' =$	·000063583	First Ap.
$\log r + z' =$	·8033982254	$z'' =$	·0000635914	Second Ap.
$\log r + z'' =$	·8033982338	$z''' =$	·000063591378	$= \Delta \log \sin x$
		$\sin x =$	9.884253966554	
		$\sin (x + \Delta x) =$	9.884317557932	

To find $\log \cos (x + \Delta x)$

$\sin x$	9·8842539666			
$\cos x$	9·8080674968			
$\tan (x + \Delta x)$	10.0763404105			
$\Delta \log \tan x$	$\bar{4}$·1873535895			
$\log s =$	$\bar{5}$·9560154634	$z' =$	·000090368	First Ap.
$\log s - z' =$	·9559250954	$z'' =$	·0000903494	Second Ap.
$\log s - z'' =$	·9559251140	$z''' =$	·000090349367	$= - \Delta \log \cos x$
		$\cos x =$	9·808067496752	
		$\cos (x + \Delta x) =$	9·807977147385	

Both results, viz. the sine and cosine of 50° 0′ 36″, may be considered as correct in all the figures; the cosine differing from the true value only by an unit in the twelfth place of decimals.

2. As an example of formulæ (F) art. 12., the logarithmic sines and cosines corresponding to $\tan(x + \Delta x) = 9{\cdot}5632889$ are required.

$$\tan(x + \Delta x) = 9.5632889$$

Next less (HUTTON's Tables) $\tan x \; (20^\circ\, 5') = 9{\cdot}5630278$

$$\Delta \log \tan x = 2611$$

	$\cos^2 x$	9·97276		$\sin^2 x$	9·53578
		9·97276			9·53578
	$\Delta \log \tan x$	3·41664		$\Delta \log \tan x$	3·41664
$\Delta \log \sin x = 2302$		3.36216	$-\Delta \log \cos x = 308$		2·48820
$\sin x = 9.5357832$			$\cos x = 9.727554$		
$\sin(x + \Delta x) = 9.5360134$			$\cos(x + \Delta x) = 9{\cdot}727246$		

15. I shall next, from the approximate values of the increments of the logarithmic sine and cosine of an angle, deduce analytic formulæ, which shall express their relations to the increments of the like functions of half the angle. In art. 1. it has been shewn, that if we neglect the third and higher powers of x, then

$$\Delta \log \sin x = \frac{\sin(2x + \Delta x)}{\sin x \sin(x + \Delta x)} \; \frac{m \cdot \Delta x}{2}; \tag{12}$$

$$-\Delta \log \cos x = \frac{\sin(2x + \Delta x)}{\cos x \cos(x + \Delta x} \; \frac{m \cdot \Delta x}{2}. \tag{13}$$

From these expressions, by substituting $\frac{1}{2}x$ for x, and $\frac{1}{2}\Delta x$ for Δx, we obtain

$$\Delta \log \sin \tfrac{1}{2} x = \frac{\sin(x + \frac{1}{2}\Delta x)}{\sin \frac{1}{2} x \sin \frac{1}{2}(x + \Delta x)} \; \frac{m \cdot \Delta x}{4}; \tag{14}$$

$$-\Delta \log \cos \tfrac{1}{2} x = \frac{\sin(x + \frac{1}{2}\Delta x)}{\cos \frac{1}{2} x \cos \frac{1}{2}(x + \Delta x)} \; \frac{m \cdot \Delta x}{4}. \tag{15}$$

By comparing the latter two expressions with the former, and observing that A being an angle, $\sin 2\,A = 2 \sin A \cos A$, we get these formulæ,

$$\Delta \log \sin \tfrac{1}{2}\,x = \frac{\cos \frac{1}{2}\,x \sin (x + \Delta\,x)}{2 \cos (x + \frac{1}{2}\Delta\,x) \sin \frac{1}{2}\,(x + \Delta\,x)}\,\Delta \log \sin x\,; \qquad \text{(G)}$$

$$\Delta \log \sin \tfrac{1}{2}\,x = \frac{\cos x \cos (x + \Delta\,x)}{4 \cos (x + \frac{1}{2}\Delta\,x) \sin \frac{1}{2}\,x \sin \frac{1}{2}\,(x + \Delta\,x)}\,(-\Delta \log \cos x)\,; \qquad \text{(H)}$$

$$-\Delta \log \cos \tfrac{1}{2}\,x = \frac{\sin \frac{1}{2}\,x \sin (x + \Delta\,x)}{2 \cos (x + \frac{1}{2}\Delta\,x) \cos \frac{1}{2}\,(x + \Delta\,x)}\,\Delta \log \sin x\,; \qquad \text{(K)}$$

$$\Delta \log \cos \tfrac{1}{2}\,x = \frac{\cos x \cos (x + \Delta\,x)}{4 \cos (x + \frac{1}{2}\Delta\,x) \cos \frac{1}{2}\,x \cos \frac{1}{2}\,(x + \Delta\,x)}\,\Delta \log \cos x\,. \qquad \text{(L)}$$

16. These expressions exhibit elegant analytic formulæ, but they cannot so readily be applied to calculation as the other formulæ, on account of the factor $\cos (x + \frac{1}{2}\,\Delta\,x)$, which enters into them all. If we assume that

$$\cos (x + \tfrac{1}{2}\,\Delta\,x) = \sqrt{\cos x \cos (x + \Delta\,x)}\,,$$

which is nearly true when $\Delta\,x$ is small, they become more simple; however, they are less accurate. When put under the same form as the expressions for $\Delta \log \sin x$, and $\Delta \log \cos x$, they will stand thus,

$$\Delta \log \sin \tfrac{1}{2}\,x = \left\{\frac{\cot \frac{1}{2}\,x \sin (x + \Delta\,x)}{\sqrt{\cos x \cos (x + \Delta\,x)}} \cdot \frac{\Delta \log \sin x}{2}\right\} \frac{\sin \frac{1}{2}\,x}{\sin \frac{1}{2}\,(x + \Delta\,x)}\,; \qquad \text{(G}'\text{)}$$

$$\Delta \log \sin \tfrac{1}{2}\,x = \left\{\frac{\cot \frac{1}{2}\,x \sqrt{\cos x \cos (x + \Delta\,x)}}{\sin x} \cdot \frac{-\Delta \log \cos x}{2}\right\} \frac{\sin \frac{1}{2}\,x}{\sin \frac{1}{2}\,(x + \Delta\,x)}\,; \qquad \text{(H}'\text{)}$$

$$-\Delta \log \cos \tfrac{1}{2}\,x = \left\{\frac{\tan \frac{1}{2}\,x \sin (x + \Delta\,x)}{\sqrt{\cos x \cos (x + \Delta\,x)}} \cdot \frac{\Delta \log \sin x}{2}\right\} \frac{\cos \frac{1}{2}\,x}{\cos \frac{1}{2}\,(x + \Delta\,x)}\,; \qquad \text{(K}'\text{)}$$

$$\Delta \log \cos \tfrac{1}{2}\,x = \left\{\frac{\sqrt{\cos x \cos (x + \Delta\,x)}}{\cos^2 \frac{1}{2}\,x} \cdot \frac{\Delta \log \cos x}{4}\right\} \frac{\cos \frac{1}{2}\,x}{\cos \frac{1}{2}\,(x + \Delta\,x)}\,. \qquad \text{(L}'\text{)}$$

Of these, formulæ (H′) and (L′), which give the increments of $\log \sin \frac{1}{2}\,x$, and $\log \cos \frac{1}{2}\,x$, from that of $\log \cos x$ are quite analogous to those investigated in the beginning of this paper, and

may be applied in the same way. The other two, which give the same things by that of $\log \sin x$, require that $\cos(x + \Delta x)$, or at least the logarithm of $\sqrt{\cos x \cos(x + \Delta x)}$ be known: now

$$\log \sqrt{\cos x \cos(x + \Delta x)} = \log \cos x + \tfrac{1}{2} \Delta \log \cos x; \tag{16}$$

therefore, when $\Delta \log \cos x$ is determined by formula (E′) art. 10. these may also be applied like the others.

17. Because $\Delta \log \tan x = \Delta \log \sin x - \Delta \log \cos x$, from equations (14) and (15) art. 15, we find (omitting a factor $\cos \Delta x = 1$ nearly).

$$\Delta \log \tan x = \frac{4 \sin(2x + \Delta x)}{\sin 2x \sin 2(x + \Delta x)} \, \frac{m \Delta x}{2}; \tag{17}$$

Hence, again, putting $\frac{1}{2} x$ for x,

$$4 \log \tan \tfrac{1}{2} x = \frac{\Delta \sin(x + \frac{1}{2} \Delta x)}{\sin x \sin(x + \Delta x)} \, \frac{m \Delta x}{4}. \tag{18}$$

This last expression, compared with (12) (13) art. 15. and (18) gives

$$\Delta \log \tan \tfrac{1}{2} x = \frac{1}{\sqrt{\cos x \cos(x + \Delta x)}} \, \Delta \log \sin x; \tag{M}$$

$$\Delta \log \tan \tfrac{1}{2} x = \frac{\sqrt{\cos x \cos(x + \Delta x)}}{\sin x \sin(x + \Delta x)} \, (- \Delta \log \cos x); \tag{N}$$

$$\Delta \log \tan \tfrac{1}{2} x = \sqrt{\cos x \cos(x + \Delta x)} \; \Delta \log \tan x. \tag{O}$$

18. The first and last of these are very simple, and they deserve attention, because the finding the tangent of the half of an angle from the sine, or from the tangent of the whole angle, occurs in the resolution of quadratic equations by the Trigonometrical Tables. Their application, however, again requires, that we have the logarithm of $\sqrt{\cos x \cos(x + \Delta x)}$.

When $\tan \frac{1}{2}(x + \Delta x)$ is to be found from $\sin(x + \Delta x)$, we may first find $\Delta \log \cos x$ by formula (C′); this gives

$\log\sqrt{\cos x \cos(x+\Delta x)}$ (16); then $\Delta \log \tan \frac{1}{2} x$ may be found by formula (M).

When $\tan \frac{1}{2}(x+\Delta x)$ is to be found from $\tan(x+\Delta x)$, we have, in the first place, $-\Delta \log \cos x = \sin^2 x \,.\, \Delta \log \tan x$, by formula F, art. 12., then,

$$\log\sqrt{\cos x \cos(x+\Delta x)} = \log \cos x - \tfrac{1}{2}\sin^2 x \cdot \Delta \log \tan x,$$

so that we may express fomula (O) in logarithms thus,

$$\log(\Delta \log \tan \tfrac{1}{2} x) = \log\{\cos x \cdot \Delta \log \tan x\} - \tfrac{1}{2}\sin^2 \Delta \log \tan x. \qquad \text{(P)}$$

In like manner, if we make Δx negative, we may find $\tan \frac{1}{2}(x - \Delta x)$ from $\tan(x - \Delta x)$ by this formula,

$$\log(-\Delta \log \tan x \tfrac{1}{2}) = \log\{\cos x(-\Delta \log \tan x)\} + \tfrac{1}{2}\sin^2 x(-\Delta \log \tan x) \quad \text{(P}')$$

the two, (P) (P′) enable us to take for x the angle expressed by the nearest even number of minutes, whether greater or less than the angle corresponding to the given tangent.

As an example of the application of both formulæ, I shall take an unfavourable case, and suppose the tangent of 89° 1′ = 11·76537923 to be given, to find the tangent of 44° 30′ 30″.

Employing the first formula, we have

$\text{Tan}(x+\Delta x)$	= 11·76537923
$\tan x$ (89°)	= 11·75807853
$\Delta \log \tan x$	= ·00730070

$\sin^2 x$ -	19·9998676	$\cos x$ -	8·2418553
$\Delta \log \tan x$	$\bar{3}$·8633645	$\Delta \log \tan x$	$\bar{3}$·8633645
	$\bar{3}$·8632321		$\bar{4}$·1052198
$\sin^2 x \cdot \Delta \log \tan x$ =	·0072985	$\frac{1}{2}\sin^2 x \,\Delta \log \tan x$ =	·0036492

$\Delta \log \tan \frac{1}{2} x$	= ·00012635	$\bar{4}$·1015706
$\tan \frac{1}{2} x$ (44° 30′)	= 9·99241975	
$\tan \frac{1}{2}(x+\Delta x)$	= 9·99254610	

By the second formula,

$$
\begin{aligned}
\text{Tan}\,(x - \Delta x) &= 11{\cdot}76537923 \\
\tan x\ (89^\circ\ 2') &= 11{\cdot}77280468 \\
\hline
-\Delta \log \tan x &= \ \ {\cdot}00742545
\end{aligned}
$$

$$
\begin{array}{lr}
\sin^2 x & 19{\cdot}9998764 \\
-\Delta \log \tan x & \bar{3}{\cdot}8707229 \\
\hline
 & \bar{3}.8705993 \\
\hline
\sin^2 x\,(-\Delta \log x) = & {\cdot}0074233
\end{array}
\qquad
\begin{array}{lr}
\cos x & 8{\cdot}2271335 \\
-\Delta \log \tan x & \bar{3}{\cdot}8707229 \\
\hline
 & \bar{4}{\cdot}0978564 \\
\tfrac{1}{2}\sin^2 x\,(-\Delta \log \tan x) = & {\cdot}0037116 \\
\hline
 & \bar{4}{\cdot}1015680
\end{array}
$$

$$
\begin{aligned}
-\Delta \log \tan \tfrac{1}{2} x &= {\cdot}00012635 \\
\tan \tfrac{1}{2} x\ (44^\circ\ 31') &= 9{\cdot}99267245 \\
\hline
\tan \tfrac{1}{2}\,(x - \Delta x) &= 9.99254610
\end{aligned}
$$

By either formula, the result is obtained true to eight places of decimals; if x were a less angle, the accuracy would be greater; we may therefore safely employ the formulæ with HUTTON's or SHERWIN's Tables: with VLACQ's *Trigonometria Artificialis*, the tangent of half the angle may be found true to ten decimal places.

THE short paper which follows this was originally intended to have formed a part of it; as, however, their objects are different, they are given separate from one another.

XI. *A proposed Improvement in the Solution of a Case in Plane Trigonometry.* By William Wallace, F. R. S. Edin. Professor of Mathematics in the University of Edinburgh.

(Read Nov. 23. 1823.)

1. In the present state of mathematical science, cultivated as it has been, with assiduity, during the two preceding centuries, it can hardly be expected that any considerable improvement remains to be made in Plane Trigonometry, one of its most elementary theories. There is, however, one case in the resolution of oblique-angled triangles, which appears to me to admit of a solution somewhat more simple and convenient than those which are commonly known; it is that in which two sides and the included angle are given to find the third side.

2. The usual way of proceeding, is to find half the sum and half the difference of the angles opposite the given sides, and from these the angles themselves; the third side may then be found in two ways, from the principle, that the sides are to one another as the sines of the opposite angles. Instead of this, I propose, that having found half the sum and half the difference of the angles in the usual way, the remaining side shall be found by either of these two formulæ:

Let the sides of a triangle be a, b, c,
and the opposite angles A, B, C,

Theorem I.

$$\cos \tfrac{1}{2}(A - B) : \cos \tfrac{1}{2}(A + B) :: a + b : c.$$

Theorem II.

$$\sin \tfrac{1}{2}(A - B) : \sin \tfrac{1}{2}(A + B) :: a - b : c.$$

By applying both formulæ, the same result will be obtained in two different ways, a thing always desirable, and better than a verification, obtained by a mere repetition of the calculation.

3. These formulæ (which are not given as new) may be briefly demonstrated as follows:

$$\text{Because } \sin A : \sin C :: a : c,$$
$$\text{and } \sin B : \sin C :: b : c;$$
$$\text{Therefore } \sin A + \sin B : \sin C :: a + b : c;$$
$$\text{also } \sin A - \sin B : \sin C :: a - b : c.$$
$$\text{But because } \sin A + \sin B = 2 \sin \tfrac{1}{2}(A+B) \cos \tfrac{1}{2}(A-B),$$
$$\text{and } \sin A - \sin B = 2 \cos \tfrac{1}{2}(A+B) \sin \tfrac{1}{2}(A-B),$$
$$\text{and } \sin C = \sin(A+B) = 2 \sin \tfrac{1}{2}(A+B) \cos \tfrac{1}{2}(A+B),$$
$$\text{Therefore, } \sin A + \sin B : \sin C :: \cos \tfrac{1}{2}(A-B) : \cos \tfrac{1}{2}(A+B);$$
$$\text{in like manner, } \sin A - \sin B : \sin C :: \sin \tfrac{1}{2}(A-B) : \sin \tfrac{1}{2}(A+B);$$
$$\text{Hence, } \cos \tfrac{1}{2}(A-B) : \cos \tfrac{1}{2}(A+B) :: a + b : c;$$
$$\text{Also, } \sin \tfrac{1}{2}(A-B) : \sin \tfrac{1}{2}(A+B) :: a - b : c.$$

4. An example will serve to shew the distinction between the common method and that proposed in this paper.

The sides of a triangle being a, b, c;
and the opposite angles A, B, C.

$$\text{There are given} \left\{\begin{array}{l} \text{side } a = 169584 \text{ feet} \\ \text{side } b = 119613 \\ \text{angle } C = 60^\circ\ 43'\ 36'' \end{array}\right\} \text{to find the side C in two ways.}$$

$a + b$	289197	5·4611938
$a - b$	49971	4·6987180
$\tan \frac{1}{2}(A+B)$	59° 38′ 12″	10·2322235
		14·9309415
$\tan \frac{1}{2}(A-B)$	16 26 0·23	9·4697477
A =	76 4 12·23	
B =	43 12 11·77	

To find side c by the common method.

sin A	9.9870362	sin B	9·8354297
sin C	9·9406644	sin C	9·9406644
side a	5·2293848	side b	5·0777784
	15·1700492		15·0184428
side c 152409·8	5·1830130	side c	5·1830131

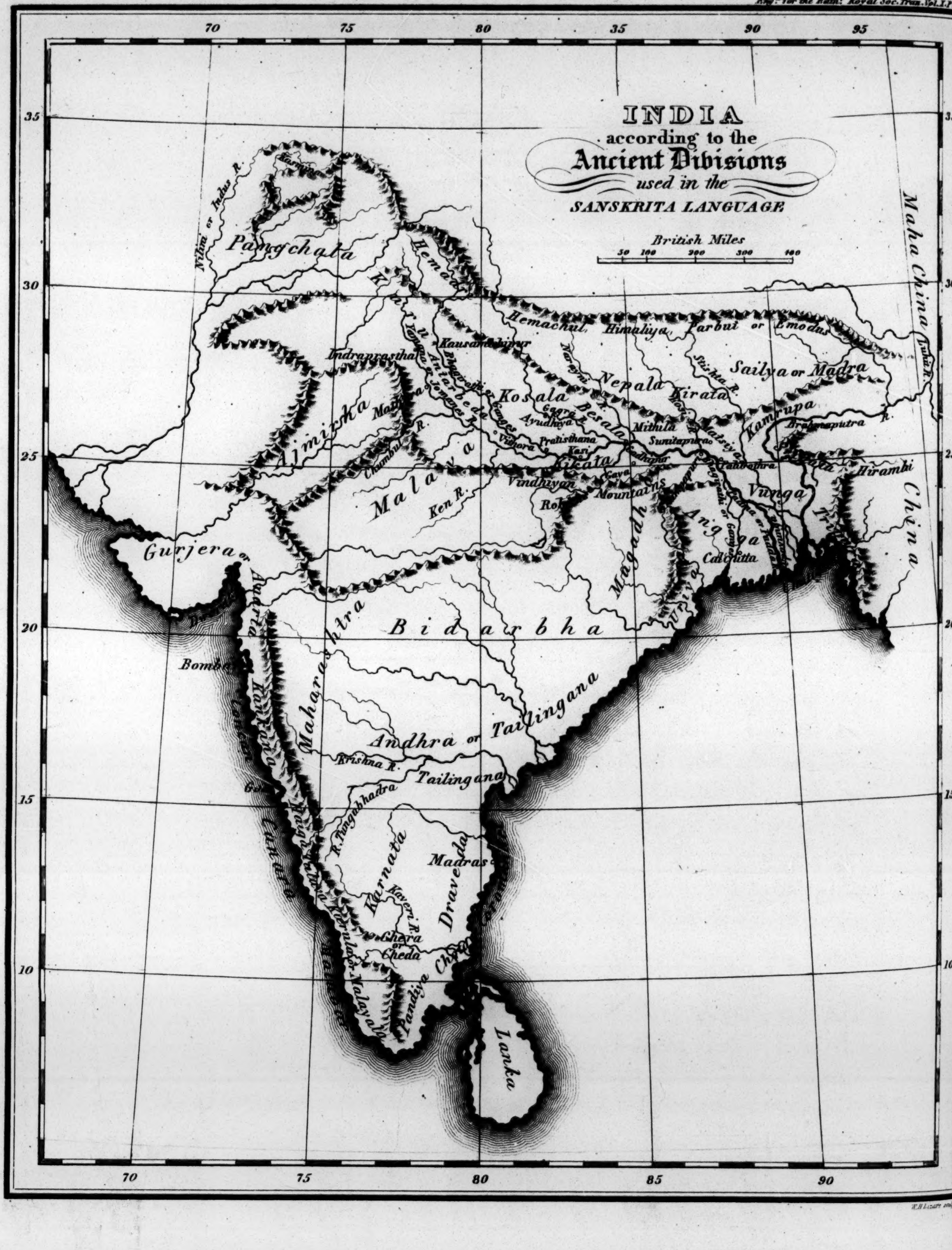
To find the side c by the method here proposed.

cos $\frac{1}{2}$ (A — B)	9·9818861	sin $\frac{1}{2}$ (A — B)	9·4516338
cos $\frac{1}{2}$ (A + B)	9·7037054	sin $\frac{1}{2}$ (A + B)	9·9359289
$a + b$	5·4611938	$a - b$	4·6987180
	15·1648992		14·6346469
c 152409·8	5·1830131	c	5·1830131

5. By comparing the two solutions and their verifications, it appears that by the common method, the Trigonometrical Tables must be opened in ten different places, and that by the new method, only five different openings are necessary, because in this last, the logarithms of $a + b$ and $a - b$ occur twice, and the cosines and sines of the angles $\frac{1}{2}$ (A + B) and $\frac{1}{2}$ (A — B) are found in the same lines with their tangents. Even if we are satisfied with a single solution, omitting the verification, the method I have here proposed still appears to deserve the preference, because, in the common way, the tables must be opened in eight places, but in the other way, only in five. Of course I do not take into account the operations necessary to find the sine and cosine from the tangent. If they be found by formula (F) art. 12., of the preceding paper, only one opening more will be required, viz. that for the logarithm of the *difference* of the log. tangent; and it is presumed that they may be found more readily than the correct values of $\frac{1}{2}$ (A — B), (which, in general, is only a subsidiary angle, and not wanted in the solution), and the log sines of the angles A and B.

PLATE VI.

Engd. for the Edinr. Royal Soc. Trans. Vol. I. P. III.



W.H. Lizars sculpt.

XII. *Some Notices concerning the Plants of various Parts of India, and concerning the Sanscrita Names of those Regions.* By FRANCIS HAMILTON, M. D. F. R. S. & F. A. S. Lond. & Edin.

(Read June 18. 1821.)

As it is my intention soon to publish, in various works on Natural History, the observations on the Botany of India which I made during my residence there, I wish to place on record an account of the opportunities which I enjoyed of making such observations, with the view of explaining to the Botanist where he may find the various collections which I made in different parts. I also wish to explain the geographical terms that I shall employ, in giving an account of the places where I found each species. For this purpose I prefer using the ancient Sanscrita names *, both as being more scientific, and as being more likely to remain permanent; for, after a lapse of many ages, they continue to be known to all Hindus of learning, while each new invasion or revolution sinks into immediate oblivion the mushroom appellations imposed by modern rulers, whether Muhammedans or Christians.

Immediately after my appointment to the Company's Service on the Bengal Establishment, I was sent with Captain SYMES to the Court of Ava, and, during the year 1795, I had an opportunity of seeing somewhat of the Andaman Islands, with a good deal of the kingdoms of Pegu and Ava. The plants of the Andaman Islands are nearly similar to those of Chatigang, of which

* A Map of India, according to the ancient divisions used in the Sanscrita language, is given in Plate VI.

I shall give a more full account. Those of Pegu nearly resemble those of the southern and eastern parts of Bengal, while those of Ava bear a stronger resemblance to the productions of Mysore. The reason of this seems to be, that the territory of Pegu enjoys much more copious rains than Ava, which, like the southern parts of what we call Hindustan, is a parched country, and, in order to bring rice to maturity, requires artificial irrigation by means of reservoirs or canals. On the way, however, between Pegu and Ava, where we approached the mountains bordering Arakan on the east, we had a vegetation much resembling that of Chatigang, and of the mountains extending from thence along the eastern frontier of Bengal, which will be afterwards described. The plants, which I collected during this journey, were transmitted, together with a good many drawings, to the Court of Directors, and were given to Sir JOSEPH BANKS, in whose collection they probably remain; but copies of most of the drawings, partly coloured, were preserved by me, and deposited in the Company's Library. I also preserved a copy of the Notes, which I took on the spot, and this will be found in the same collection.

In 1796, 1797, and part of 1798, I was stationed at Lukhipur, in the south-eastern part of Bengal, and in the ancient kingdom of Tripura. My time was there much occupied in describing the fishes of the country; but I took many descriptions of plants, which are also deposited in the Company's Library; but I did not preserve specimens. I corresponded, however, very frequently with Dr ROXBURGH, and transmitted to him whatever he thought would be acceptable, learning, at the same time, what both he and KŒNIG called various plants.

In spring 1798, by the desire of the Board of Trade at Calcutta, I visited the district of Chatigang, which, together with that of Komila, formed the chief part of the ancient kingdom of Tripura, and I afterwards skirted the hills of Komila, where the

tribe of Tripura still maintains a kind of independence. Here I had a full opportunity of examining the splendid vegetation of the well watered districts of Farther India *(extra Gangem)* which bounds the extensive Gangetic plain on the east, and extends south from what we call China to the Ocean. It must be observed, however, that this Farther India, as it has been called, is the proper China of the Hindus, from whom we derived the word, while, what we name the Chinese Empire, the Hindus call Maha China, or the Great China.

The largest portion of this Farther India, or Southern China, is mountainous and well watered; but its mountains nowhere rise to an alpine elevation, and, owing to a copious supply of moisture, and a deep soil, are, in general, covered to the summit with lofty forests. I have already mentioned, that a great part of the proper kingdoms of Pegu and Ava differs a good deal from the general appearance of the neighbouring countries, the former resembling more the southern plains of Bengal, and the latter the southern peninsula of India; but by far the greater portion of this Farther India, in its vegetable productions, resembles Chatigang; and what Rumphius called *India aquosa*, or the immense Eastern Archipelago, including the Andaman and Nicobar islands, may be considered as belonging to the same vegetable arrangement. Of this the most prominent feature is a tendency in trees of considerable size to twine round others, forming thus forests almost totally impervious. These twining trees, the *Funes sylvestris* of Rumphius, are often thicker than the human body, and extend to great distances, overwhelming the most lofty and vigorous woods; and so strong is the tendency to this kind of vegetation, that some even of the Palmæ (Calamus, L.) a tribe in general remarkable for erect stiffness, are here climbers, and, after overtoping the highest trees, again drop branches to the earth, which take root, and climb up the trees that are adjacent; and thus, with other thicker, though less powerfully armed

climbers, form a mat which becomes almost impenetrable. This thick vegetation produces a delightful coolness, and preserves a moisture that encourages the growth of numerous and beautiful parasitical plants, Filices, Aroideæ and Orchideæ; but renders the climate rather sickly to constitutions unaccustomed to such a moisture. In this fine region, the valleys between the hills are uncommonly fertile, and, being well watered, produce abundant crops of rice, the grand source of nourishment for the inhabitants, although the tuberous Aroideæ and Dioscoreas, both very nutritious, may be considered as the proper offspring of this territory, where they thrive with an uncommon vigour and variety. In this country, even the unoccupied wastes have a luxuriance of vegetation, that renders them almost equally impervious with the forests; and grasses, mostly of the genus Saccharum, shoot up with a prodigious luxuriance and thickness. They generally exceed six feet in height, and often reach to twice that elevation.

The trees that are most common in this territory, are of the orders of Urticæ, Euphorbiæ, Terebinthaceæ, Magnoliæ, Meliæ, Guttiferæ, Sapotæ, Vitices, and Eleagni, and, together with the Palmæ, Bambusæ and climbers, form the great features of vegetation, which are of a totally exotic appearance to the European, having scarcely any thing to recall the memory of his native scenery; yet still highly pleasing, not only from their novelty, but also from their beauty and grandeur. Notwithstanding this great difference of general appearance, several of the trees have an affinity with those of Europe, and the woods contain an Æsculus, and several Querci and Coniferi.

The specimens which I collected during this journey were transmitted to Sir JOSEPH BANKS, in whose collection I saw them in the year 1806, and there they no doubt will still be found.

Soon after my return from Chatigang, I was removed to Baruipur, a station near Calcutta, where I chiefly employed my leisure in describing fishes. Still, however, I continued to collect whatever appeared rare for Dr Roxburgh, especially during several journeys which I made through the great forests that occupy the islands formed by the estuaries of the Ganges. These dreary woods, half inundated by the tides, and skreened by banks of offensive mud, afford but little scope to the botanist. The variety of vegetables which they contain is by no means great; and the danger in attempting to collect them, by landing where tigers are so numerous and ravenous, is very great. I believe, however, that in the various journies which I made between Calcutta and Lukhipur, and from Baruipur, through these woods and islands forming part of the ancient kingdoms of Vanga, Upavanga, and Angga, I had an opportunity of describing most of their vegetable productions. Mangroves of various kinds, including Rhizophora, Ægiceras, Avicennia, Sonneratia, and Heritiera, especially the latter, form the predominant feature of these woods; but they are ornamented with curious Convolvulaceæ and Apocineæ, with many parasitical Filices, and some elegant Lycopodiums and Lichens, not remarkable, indeed, for variety, but of great size and beauty.

The cultivated parts of this Delta of the Ganges, as it has been called, are not more favourable to the botanist than the wastes. The plough or hoe occupies almost every spot, one rice-field succeeds another, and the houses are buried among groves of Mangifera, Artocarpus, and Bambusa, intermixed with Palmæ, and are only kept above water, by being raised on the banks thrown up by digging ponds. In this territory the wastes are generally covered with reedy grasses, almost as lofty as those of Tripura. The whole aspect, indeed, of the country, and of its vegetation, is strange and foreign to an European, unless to a

Hollander. For four months in the year every field swarms with fish, and at all times the only conveyance is by boats.

During my stay in this part of the country I made few botanical observations, except by communications with Dr Roxburgh. I, however, transmitted a few descriptions and drawings to Sir J. E. Smith, with whom they still remain.

During the year 1800, I was employed by the Marquis Wellesley to examine the state of the country which he had lately taken from Tippoo Sultan, and of the province which Europeans call Malabar. I landed at Madras (Chinapatana of the natives), and travelled through the territory belonging then to the Nabob of Arcot, which Europeans call the Carnatic, but it is the Draveda of the Hindus, bounded on the south, at the mouth of the Kaveri, by Chola, which Europeans call Tanjore, and to the north by Andhra, the sea-coast of which by Europeans is usually called the Circars, as having once been divided into five districts (Circar), which were early ceded to Europeans by the Muhammedan princes of Andhra or Tailingana. The coasts of Chola, Draveda, and Andhra are usually included by Europeans under the denomination of Coromandel, a name totally unknown to the natives, who consider it as English, and from which we have several plants named Coromandeliana, as from the English word Madras, with the addition of Patana (City) we have Maderaspatana, as if plants grew in the streets. Both names should be avoided as inconveniently long, as well as devoid of meaning in any language.

On leaving Draveda, and ascending to the elevated region, lately under the dominion of Tippoo Sultan, I entered the ancient Hindoo territory, called by them Karnata (Latine, Carnata), but usually known to Europeans by the name of Mysore, from the town where its princes for some generations resided. Having examined this and the skirts of the interior of Andhra, I descended again to the low country by the south, and exa-

mined the country west from Chola, which the natives call Chera or Cheda, but which Europeans, from a town in it, call Coimbetore (Coiamatura). Chera as well as Chola is bounded on the south by the country which the natives call Pandiya, extending from near the Kaveri to the Southern Ocean. The northern parts of this, towards Chera, I had an opportunity of examining. The vegetation of all these countries is nearly similar. The elevation of Mysore above the others, although probably about 3000 feet of perpendicular height, produces no great change. The temperature is no doubt somewhat lower, and more agreeable to European feelings; but the aspect of the upper country is not materially different from that of the lower. Both labour under a scarcity of rain, so that artificial irrigation from reservoirs or canals is necessary for the production of rice, which, in the low country especially, is the staple article of food, although both there and in the higher country the rainy season produces crops of miserable small grains, such as Eleusine Corocanus, Panicum Italicum, and Panicum miliaceum, that are used by the natives as a succedaneum for rice. These crops have little of an European appearance; nor do the orchards and gardens heighten the resemblance. The fruit trees round the villages consist chiefly of the Mangifera, Citrus, Bassia, Artocarpus, Eugenia, Elate, and Borassus, while the kitchen gardens require to be watered by machinery from wells. The general appearance of the country is sterile, the rock projecting in a great many places, while, during the greater part of the year, the grass is entirely parched up from want of moisture; and even in the rainy season the grass is not longer than is usual in Europe. In the woods, the trees are still more stinted than those of Europe, and consist in a large proportion of wild prickly dates (Elate sylvestris) and Bambusæ, with trees of the Leguminosæ, especially such as have prickles, and of the Rhamni. Even the thickets consist chiefly of bushes of the Leguminosæ, and of the Rhamni

and Caparides, almost all armed with prickles or thorns, while the fences are chiefly of naked Euphorbiæ (Antiquorum and Tirucalli). The most common trees besides the Leguminosæ and Rhamni, belong to the tribe of Eleagni and the genus Grewia: and the most common herbage consists of small Cyperus, Scirpus, Andropogon, Convolvulaceæ, Acanthaceæ, and Leguminosæ, especially Hedysarum, Crotolaria, and Indigofera, so that the vegetables have little in common with those of Europe, especially of its northern parts. With the more barren parts of southern Europe there is more resemblance, the Rhamni and Caparides being common to both.

After examining these countries of rigid vegetation, as it may be called, I passed through the gap in the Animaliya or Elephant Mountains, and entered the province called Malabar by Europeans, but Kærula and Malayala by the natives. These, indeed, consider Malabar as an English word, meaning the whole sea-coast between Cape Comorin and Surat, which seems to be the fact. We ought, therefore, to call the province of Malabar by one or other of the native appellations. The territory called Kærula by the natives, extends from the southern extremity of India to almost the latitude of 12½ degrees North; but this includes a portion of the English province of Canara; and it extends from the summits of the mountains to the sea. In its vegetable productions and appearance, it more resembles Chatigang and the mountains of Farther India than the adjacent territory of rigid vegetation; but it is better cultivated, contains more plantations, especially of Palmæ, and, the rock projecting more, the vegetation is not quite so luxuriant. It has, however, perhaps still less of an European appearance, none of the Amentaceæ nor Coniferæ being found in its woods. The Dutch, however, have introduced many fine trees from the Eastern Islands, and the Portuguese some from the West Indies; both of which give a considerable variety to its plantations, and few countries possess a ve-

getation so elegant, prospects more grand and beautiful, and a climate more genial. Its highest mountains, although of considerable height, perhaps 6000 feet perpendicular, have nothing of an alpine appearance, but produce a moisture and coolness that extends a more vigorous vegetation to the adjacent country above.

Nearly connected with Kærula, and little different from it in vegetable productions, is Ceylon, the Taprobana of the Romans, and the Lanka of the ancient Hindus. In 1815, I had an opportunity of a cursory examination of its southern end, and saw sufficient to indicate, that, in general aspect at least, it does not materially differ from Malayala.

North from Kærula, and, as I have said, including a portion of it, is the extensive English province of Canara, a word of doubtful origin, and supposed by the natives to be English. The Hindus divide it into four territories : 1st, Part of Kærula or Malayala, extending to about 12° 28′ North latitude ; 2d, Tulava, extending from thence to about 13° 35′ N. ; 3d, Haiva or Haiga, extending to about 14° 38′ N. and Kankana (Latinè Cancana) extending to the Portuguese territory of Goa ; but this, as well as all the sea coast to near Bombay, are included in the territory which the Hindus call Kankana. These countries, like Malayala, extend from the summit of the mountains to the sea, and scarcely differ in appearance or vegetable productions from that territory ; but they are rather hotter and drier, and their vegetation is rather less vigorous, approaching more nearly to the rigid thorny nature of that prevailing towards the East.

The specimens of plants which I procured during this journey, suffered much by the carelessness of those who were entrusted in conveying them from the ship to Calcutta ; but such as they were, they were given to Sir J. E. Smith, together with a good many drawings, and both remain in his collection. The notes which I took have been deposited in the Company's Li-

brary. Some duplicate specimens were given to A. B. LAMBERT, Esq. and I think that Sir J. E. SMITH has a copy of the notes: of this, however, I am not certain.

Soon after my return from the south of India, I was sent to Nepal along with the embassy conducted by Captain KNOX. Having proceeded by water to Patna, I passed, by easy stages, and with many halts, through the ancient territory of Besala, now called Sarun; and through a portion of Mithila now called Tirhut. There I carefully examined and collected such plants as were in flower; and, on the 1st of April 1802, I ascended into Nepal, where I remained nearly twelve months, delighted with the variety, beauty, and grandeur of its vegetable productions, of which I procured many specimens, descriptions and drawings, all of which I gave to Sir J. E. SMITH, only reserving specimens, where there were duplicates, for Mr LAMBERT. I afterwards had an opportunity of procuring many specimens from the same quarter, and of making many observations on these plants, which I may have occasion to use under the disagreeable circumstance, that I may have described the same plant under different names, among those given to Sir J. E. SMITH, and among those which I afterwards procured; but under the circumstances already mentioned, this was unavoidable. For an account of the appearance of the vegetables in this interesting region, I may refer to the Account of Nepal which I have published.

Soon after my return to Calcutta in 1803, I was appointed Surgeon to the Governor-General; and the leisure I then had for the study of Natural History, was chiefly employed in superintending the Menagerie founded by the Marquis WELLESLEY, and in describing the animals there collected. I returned to England with this distinguished Nobleman in the end of 1805, and in 1806 was appointed by the Court of Directors to make a statistical survey of the territory under the Presidency of Fort William, usually in Europe called Bengal; but containing many ex-

tensive regions besides Bengal, taking that even in the most extended sense of the Mogul province of the name; for in Hindu geography, Vanga, from whence Bengal is a corruption, is applied to only the eastern portion of the Delta of the Ganges, as Upavanga is to the centre of this territory, and Angga to its western limits.

I commenced this survey after the rainy season of 1807, with the English district of Dinagepore (Dinajpura), forming part of the ancient kingdom of Matsiya, bounded by the Mahananda on the west, by the Korataiya (Latinè Coratæa) on the east, by the mountains on the north, and by the Padma or eastern branch of the Ganges on the south. This district is not very favourable for the botanist, being in general highly cultivated; but its southern parts, especially round the ancient city of Purua, are woody, and yielded a considerable increase to my collection.

In spring 1808, having finished the survey of Dinagepore, I passed through the English district of Rungpur (Ranggapur), the Kamrupa of the ancient Hindus, and having examined the north-eastern wastes of that territory, where I added much to my botanical stores, I halted for the rainy season at Goyalpara (Latinè Goalpara). This place, situated at the northern extremity of the mountainous district, which bounds the Gangetic Plain on the east, afforded me most ample employment as a botanist, producing a variety of beautiful and rare plants, almost equal to that of Nepal; and, with my journeys to Ava and Chatigang, enabled me to form a proper estimate of the vegetable productions of Farther India *(ultra Gangem)*, the China of the Hindus, and which I have already described.

With the fair weather of 1808 I recommenced the survey of the Rungpur district, where I found an excellent field for a botanist, as it contains many wastes. As the rainy season of 1809 approached, I retired to a house near the town of Rungpur, and there continued, in a situation not very favourable for a botanist,

until I had time left only to convey me to Purneah (Puraniya), before the fair weather of 1809 should commence.

The English jurisdiction of Purneah (Latinè Purania) forms a part of the ancient Hindu kingdom of Mithila, with a small portion of Angga around the ruins of Gaur; but my journey, during the dry season, added little to my botanical stores. This, however, was amply recompensed by my stay, during the rainy season 1810, at Nathpur, on the frontier of the Kiratas or Ciratas, subject to Nepal, from whence, as well as from the forests in the northern parts of Mithila, I procured a great variety of rare and curious plants.

In autumn 1810, so soon as the weather cleared, I proceeded to the district of Boglipore (Bhagulpur), the eastern part of which is included in the ancient Hindu kingdom of Angga, while its western portion is in Magadha, and the portion on the northern banks of the Ganges is partly in Angga, partly in Mithila. The greater portion of this district being waste, was very favourable to me as a botanist, and I had here an opportunity of extending my knowledge of the rigid vegetation of the Vindhiyan Mountains, which the Hindus consider as bounding the Gangetic plains on the south, and as extending from the southern banks of the Ganges to the Southern Ocean. These hills are here much lower than the parts of the same mass which I examined in the south; but their vegetable productions are nearly the same, and have a similar rigid thorny appearance; but, the rains being more copious, the vegetation is not quite so much stinted, although it is very far from being so luxuriant as that towards the east or north.

The rainy season 1811 I passed at Mungger, where the vicinity of the hills gave me a considerable increase to my stock of plants, and I employed a Hindu physician, not deficient in learning, to point out the plants which he considered officinal, and to give me both their Sanskrita and Hindu names, which I compa-

red with those given to the same plants by the ignorant people who collect and vend drugs.

In the following dry season 1811–12, I examined the jurisdictions subject to the magistrates of the cities of Patna and Gaya, both included in the ancient kingdom of Magadha, which for many centuries before the Muhammedan invasion, was considered the chief seat of Hindu power and glory, so that its princes were indifferently called Kings of Magadha and of Bharatkanda, or the Land of Virtue, the name by which the Hindus fondly call the territory occupied by their race, the descendants of Brahma. In these districts I had a farther opportunity of making myself acquainted with the rigid vegetation of the Vindhiyan Mountains, and, during my stay at Patna, in the rainy season 1812, I extended my knowledge of the officinal plants of India, by consulting the same physician and the druggists of Patna.

In the dry season 1812–13, I examined the jurisdiction under the magistrate of Shahabad, forming a great part of the ancient Hindu kingdom of Kikata (Latinè Cicata); and here I completed my knowledge of the vegetation of the Vindhiyan Mountains, which, the farther west I proceeded, rose to a greater elevation, were more rocky, and communicated to their vegetation more and more of the rigid and thorny nature of that produced on the arid hills and mountains of Draveda, Karnata, and Chera.

Soon after the rainy season of 1813 commenced, I embarked at Chunar, and proceeded up the Ganges and Yamuna (Jomanes Plinii) or Jumna to Agra, and thus had an opportunity of examining the plants on the banks of these rivers, passing along a portion of the ancient kingdom of Malava (Malwa) on the east of the Yamuna river, near the Ken (Cainas Plinii) and Chumbul rivers, and then proceeding through the centre of the ancient kingdom of Kuru, which, in the earlier part of the Hindu government, was the chief seat of power and glory, restored to it afterwards by the Muhammedan conquest, and only lately restored to

Angga by British valour and prudence; for, in the time of Alexander, Angga was no doubt the chief seat of Hindu power, as Palibothra seems to have been seated opposite to Rajamahal in Angga, although on the skirts of Magadha, which in latter times was the great seat of authority.

Before the end of the rainy season I returned down the rivers, and ascending the Gagra, entered the district of Gorakhpur, forming a considerable portion of Cosala, the territory of the powerful Family of the Sun, who reigned at Oude (Ayudhiya). During the dry season 1813–14, I remained in the district of Gorakhpur, where I made large additions to my botanical observations, both from the forests of the country, and from the neighbouring parts of Nepal, from whence I procured many plants.

When the rainy season commenced I again embarked, and proceeded up the Ganges to Futehgar, where I had again an opportunity of examining the vegetable productions of the ancient kingdom of Kuru, through the centre of which the Ganges passes; for it includes both banks of the Ganges and Yamuna, being bounded on the east by Kosala, and on the west by Pangchala, now called the Punjab, or the country watered by the five rivers joining the Indus from the north-east.

Having thus examined a considerable portion of the Gangetic plain, always considered the proper seat of the Hindu race, descended from a colony of civilized persons calling themselves sons of Brahma, who in the earliest ages settled at Vithora (Betoor Rennell), and gradually extended their power over what is now called Hindustan, I shall proceed to give some general account of the vegetation of this fertile tract, which, without any thing that can be called a hill, extends from the Indus to the Eastern Ocean, and from the Vindhiyan to the Himaliya mountains.

This plain, extending in length about fourteen degrees of longitude, in the middle latitude of 25°, and in breadth from two

to four degrees of latitude, seems to derive a large proportion of its vegetation from the neighbouring hills ; but grasses, especially Bambusa, Saccharum, Andropogon, Apluda, and Panicum, together with the allied tribes of Cyperoideæ, form a larger and more marked feature than trees or shrubs. On the whole, the rigid and thorny vegetation of the Vindhyan mountains seems more suited for the plain than the more ornamental vegetation of either the Eastern or Himaliya mountains. Near both these, however, their plants have made considerable encroachments, and communicate a change of appearance to the adjacent plains, especially towards the east, where the air is vastly cooler and moister than farther west.

I have already mentioned the appearance of the Gangetic Delta, which, on the whole, has a strange and exotic appearance to the European traveller. As we advance, however, to the north, and still more as we proceed west, notwithstanding the intense heats of the summer, the vegetation appears more of an accustomed form. Wheat, Barley, Pease, and Rape-seed form by far the largest proportion of the crops, and we observe fields of Potatoes and Carrots, while the Palmæ and Bambusæ disappear from the plantations, and the gardens produce the Vine, the Fig, the Apple, and the Plum, with many flowers common in Europe, and the thickets contain much of the wild Rose. Still, however, even in Kuru, the Mangifera, the Eugenia, the Calyptranthes, the Fici (religiosa and bengalensis) the Rhamni, and the exotic crops produced in the rainy season (Oryza, Holcus, Panicum, Paspalum, Dolichos) with the want of the Coniferæ and Amentaceæ in the plantations, remind us sufficiently that we are not in Europe.

I now was exhausted by a long continued exertion ; the observation of plants making but a small part of my duty, and I required to pass the remainder of my days at peace in my native

climate. I accordingly returned to Calcutta, to prepare for my journey; and, in the mean time, on the death of Dr ROXBURGH, took charge of the Botanical Garden, having been appointed his successor by the Court of Directors. While preparing for the journey, I was deprived by the Marquis of HASTINGS of all the botanical drawings which had been made under my inspection during my last stay in India, otherwise they would have been deposited, with my other collections, in the Library at the India House. By this ill-judged act of authority, unworthy of this Nobleman's character, the drawings will probably be totally lost to the public. To me, as an individual, they were of no value, as I preserve no collection, and as I have no occasion to convert them into money.

In February 1815 I embarked for Europe, and in September presented my whole collections to the Court of Directors, with an order from the Lords of the Treasury for their being delivered free from duty,—an order which was granted with the utmost liberality and urbanity.

XIII. *On a New Species of Double Refraction, accompanying a remarkable Structure in the Mineral called Analcime.* By

DAVID BREWSTER, LL.D. F. R. S. Lond. & Sec. R. S. Ed.

(Read Jan. 7. 1822.)

THE Mineral called *Analcime* or *Cubizite* has been ranked by HAÜY among those crystals which have the *Cube* for their primitive form; an opinion which has been adopted by all succeeding mineralogists. No distinct cleavage-planes, however, so far as I can learn, have been observed in it *. Crystallographers presumed that such planes must exist, and, allowing conjecture to supply the place of observation, they considered Analcime as differing in no respect from other crystals of the same series. This opinion was first rendered doubtful by the observation, that at thicknesses of $\frac{1}{25}$th of an inch, it displayed a considerable action upon polarised light †; but though, from the tessular form of the mineral, this fact indicated something singular in its organization, yet, owing to the great difficulty of obtaining proper specimens, I have been baffled in repeated attempts to investigate its structure.

* Mr PHILLIPS observes, "that there have been *occasional appearances* of cleavage-planes parallel to the faces of the cube."—*Mineralogy*, 1823, p. 129. M. MOHS describes the cleavage as *hexahedral*, but *imperfect*. HAÜY does not seem to have observed any cleavage. In the first edition of his *Traité*, published in 1801, he says, *Les cristaux diaphanes offrent seuls quelques indices de lames paralleles aux faces du cube*, tom. iii. p. 181; but in the second edition, published in 1822, he has struck out this observation. See tom. iii. p. 170. In the transparent and perfectly crystallised specimens, I cannot find any cleavages. I consider Analcime, therefore, as a mineral *without cleavage;* and if cleavage-planes are discovered, they will no doubt be found in the direction of the planes of no polarisation.

† *Philosophical Transactions*, 1818, p. 255.

Having lately received from the Reverend Dr Fleming of Flisk, some very transparent crystals of Analcime from the Macdonalds' Cave in the Island of Eigg, and having also been favoured with a very fine specimen from Montecchio Maggiore in the Vicentine, through the liberality of Mr Heuland, I have been enabled to resume the inquiry, and have obtained the results which it is the object of this paper to describe.

The most common form of the Analcime is the *Icositetrahedron*, a solid contained by *twenty-four* equal and similar trapeziums, and formed by three truncations on the eight solid angles of the circumscribing cube, inclined 144° 44′ 8″ to each of its faces, and 146° 26′ 33″ to one another, (See Fig. 1. Plate VII.). If we suppose this cube to be dissected, as in Fig. 2., by planes passing through all the twelve diagonals of its six faces, it will be reduced into *twenty-four* irregular tetrahedrons. The same planes divide the icositetrahedron into twenty-four similar *pentahedrons*, two of whose planes are placed at right angles to each other, having for their common section one of the axes of the solid, while a third, equally inclined to these two, and forming an angle of 45° with their common section, passes through the centre of the icositetrahedron. The other two planes are halves of two of the adjoining trapezia, which form the surface of the general solid.

If we transmit polarised light in a direction perpendicular to any of the faces of the cube, we shall find that all the dividing planes now mentioned, are planes of no double refraction or polarisation, that is, that they consist of an infinite number of axes parallel to the four axes of the solid *.

When any of the axes of the cube are in the plane of primitive polarisation, the tints will disappear, and continue invisible

* These planes correspond with the double set of cleavage-planes which, according to Haüy, are found in Amphigene.

Fig. 2.

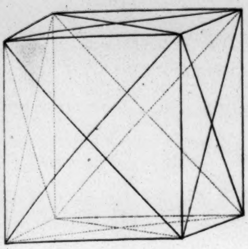


Fig. 1.

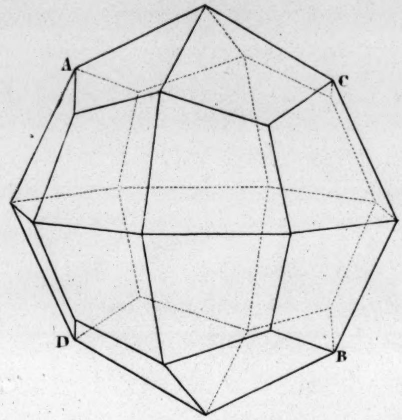


Fig. 3.

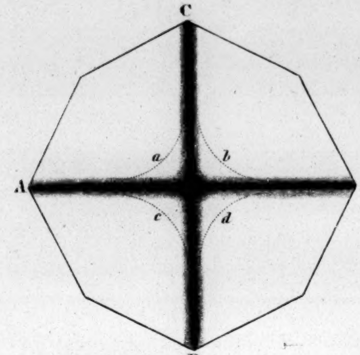


Fig. 4.

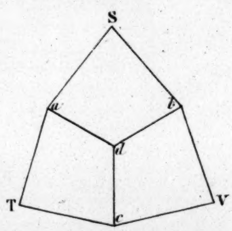


Fig. 9.

Fig. 5.

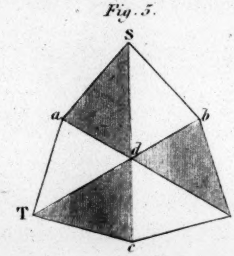


Fig. 6.

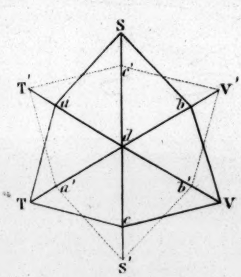


Fig. 7.

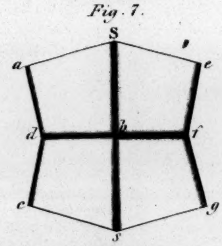


Fig. 8.

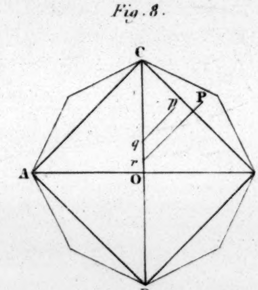


Fig. 10.

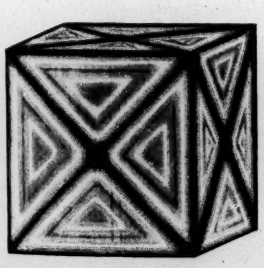


Fig. 11.

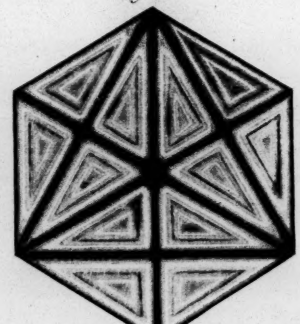
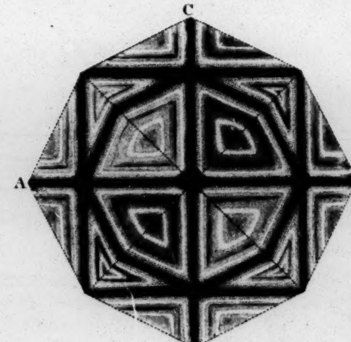


Fig. 12.



W. H. Lizars sculpt.

while the crystal is made to revolve round that axis; but when the axis is inclined 45° to that plane, or when the diagonals of any of the cubical faces are in the plane, we observe a black cross in the directions AB, CD, Fig. 1., separating four luminous sectors covered with the tints of polarised light.

If ACBD, Fig. 3., representing a piece cut off the summit of the icositetrahedron by a plane perpendicular to one of the axes, is exposed to polarised light, it will exhibit, of course, the black cross AB, CD, and the four sectors of polarised light. If the crystal is now turned round CD as an axis, so that the part A is brought near the eye, and B retires from it, the black cross opens at the centre, and assumes the form of two curves A *a* C, B *d* D; but when B is brought near the eye, and A retires, these curves have the position B *b* C, D *c* A.

When a slice is cut from the summit with three planes, or that which corresponds to one of the diagonals of the cube, as shewn in Fig. 4., the three planes of no polarisation *a d*, *b d*, *c d*, are distinctly seen. If the line *c d* is placed in the plane of primitive polarisation, the sector S *a d b*, opposite to it, becomes dark, and the same is true of the other lines *a d*, *b d*. If, instead of a slice of the crystal, we use a complete icositetrahedron, and look along the diagonal of the cube, we observe *six* sectors, as in Fig. 5. The reason of this is, that the three planes at the opposite end of the diagonal have an inverse position, the three edges of the one, corresponding with the diagonals of the three trapezia in the other, as shewn in Fig. 6. If the analysing plate has its plane exactly perpendicular to that of primitive polarisation, all the six sectors are equally whitish in minute crystals; but by turning the plate to the left, three of the alternate sectors become dark, and by turning it to the right, the other three become dark. The polarisation is a minimum along this and all the other diagonals. When the plane T *a d c* of the slice shewn in Fig. 4. is inclined, so that *d* is brought nearer the eye, and

T retires, the tints rise in the scale, and *vice versa.* When the light is transmitted obliquely, the lines *c d*, &c. disappear.

When a slice is cut off a summit with four planes, corresponding to the edges of the cube, as in Fig. 7., the lines of no polarisation *a d*, *c d*, *b d*, *b f*, *e f*, *g f*, are visible. The lines *a d*, *c d*, and *e f*, *g f*, become sharper and narrower the more the incident ray approaches to parallelism with the diameters passing through *d* and *f*. When S *s* or *d f* are in the plane of primitive polarisation, the tints all vanish, because one of the axes is then in that plane. When S *s* is inclined, so that S recedes from the eye, the tints in S *a b e* rise, and those in *s d b f* fall, and *vice versa.*

In order to determine the character of the tints, we have only to cross them with the axis of a plate of any crystal the character of whose action is determined. When the polarised tints shewn in Fig. 3. are crossed by a plate of sulphate of lime, having its axis inclined 45° to the arms of the black cross AB, CD, the tints all descend in the scale, and consequently the polarising action of the crystal is *negative* in relation to each of the four axes of the icositetrahedron. In like manner, when the axis of the sulphate of lime crosses any of the three sectors in directions passing through *d*, Fig. 4., the tints in the sector thus crossed descend in the scale. When the axis of the sulphate of lime is placed in the direction S *s*, Fig. 7., the tints likewise fall.

In all these different directions, the tints polarised by the crystal are exactly those of Newton's scale, and have all the properties of the tints of moveable polarisation.

From an attentive consideration of the preceding experiments, it is obvious that the phenomena of the tints exhibited in any individual sector COB, Fig. 8., have no relation to the axis of the icositetrahedron passing through O, considered as an axis of double refraction. The axis of polarisation of every portion in

each sector, as COB, is, on the contrary, perpendicular to the line CB, or parallel to one of the rectangular axes of the icositetrahedron, which is perpendicular to the axis passing through O. The tint of any point p, for example, does not depend upon its distance pO from O, but upon its distance $p\,q$ from the nearest plane of no polarisation, taken in a direction perpendicular to CB. Calling T, then the tint, as determined by experiment, of any point P, whose distance P r, taken in the manner now mentioned, is D, we shall have the tint t at any other point p whose distance $p\,q$ is d,

$$t = \frac{\mathrm{T}\,d^2}{\mathrm{D}^2},$$

the thickness of the crystal being supposed equal at both these points. The polarising structure, therefore, of any two opposite sectors, is the same as if it were produced by compression, the axis of pressure coinciding with the axis of the icositetrahedron perpendicular to CB, and to the axis passing through O.

This remarkable structure produces a distinct separation of the ordinary and extraordinary images of a minute luminous object, when the incident ray passes through any pair of the four planes which are adjacent to any of the three axes of the solid. The least refracted image is the extraordinary one, and consequently the doubly refracting force is *negative*, like that of *Calcareous Spar*, in relation to the axis to which the refracted ray is perpendicular.

In order to convey some idea of the remarkable structure of Analcime, I have represented the Planes of no Double Refraction and Polarisation, and the tints of the intermediate solids, in Figures 9, 10, 11, and 12. The dark shaded lines are the planes of no double Refraction, and the faint shaded lines represent the tints. The appearances, however, shewn in these figures, can never be seen by the observer at once, but they will assist the reader in following the experimental details, and in forming

a correct notion of the phenomena. In Fig. 10. I have represented the *Cube*, seen in perspective; and in Figure 11. the *Cube*, projected on a plane perpendicular to one of its diagonals. Figure 9. represents the *Icositetrahedron* in perspective; and Figure 12. the same solid projected on a plane perpendicular to one of its axes.

One of the most important results of these experiments, is the singular distribution of the doubly refracting force, not merely in the crystal considered as a whole, but in each of the separate pentahedrons which compose it. In all other crystals in which the laws of double refraction have been studied, the axis to which the doubly refracting force is related has no fixed locality in the mineral. It is a line parallel to a given line in the primitive form, and every fragment of a crystal, however minute, possesses this axis, and all the optical properties of the original crystal, however large. The property of double refraction, in short, in regularly crystallised substances, resides in the ultimate particles of the body, and does not depend upon the mode in which they are aggregated to form an individual crystal.

In Analcime, on the contrary, we have planes of no double refraction, having a definite and invariable position, and we may even extract a portion of each separate pentahedron which has no axis at all.

Nor has the doubly refracting structure of Analcime any relation to that of composite crystals, such as the *Bipyramidal Sulphate of Potash* *, which consists of several individual rhomboidal prisms, beautifully combined to form a regular geometrical solid, or that still more complicated mineral *Apophyllite*, where an individual crystal with one axis is symmetrically united with several individual crystals with two axes, so as to constitute a regular crystal †. In these, and other cases, each individual crystal

* See *Edinburgh Philosophical Journal*, vol. i. p. 6.

† See *Edinburgh Philosophical Journal*, vol. i. p. 1.; and *Edinburgh Transactions*, vol. ix. p. 317.

that enters into the combination, retains its own character, and, considered by itself, possesses the ordinary properties of double refraction.

The Analcime partakes of the character of other composite minerals, in so far as it is made up of twenty-four individual pentahedrons; but each pentahedron possesses a new species of double refraction, which has been found in no other crystal. This structure resembles, to a certain degree, that of rectangular plates of glass, while in the act of being heated, in having the phenomena related to planes of no double refraction; but the resemblance goes no farther, as the structure of the glass depends upon its external form, and the planes of no polarisation change their position with the outline of the plate. In Analcime, on the other hand, the structure is permanently fixed, and has no relation whatever to the external shape of the fragment.

In the absence of more striking analogies, we may consider this structure as resembling that which is produced by hardening isinglass, when in a state of compression or dilatation. In this case the isinglass retains a fixed doubly refracting structure, related to the axis of compression or dilatation; and if it were cut into pentahedrons, similar to those of the Analcime, we might combine them together, so as to imitate, at least in the direction of one of the axes, the phenomena exhibited by the mineral.

The property which has now been described becomes an infallible and easily applied mineralogical character for Analcime. However shapeless be the fragment, and however much obliterated be its external faces, its action upon polarised light will instantly determine whether or not it belongs to this species.

Haüy first observed in Analcime the singular circumstance of its yielding no electricity by friction, and he even derived its specific name from its want of this property. If we consider that

the crystal is a combination of solids of variable density, and separated from one another by numerous intersecting planes or nodes, where the variations of density change their direction, we may ascribe to this cause the difficulty with which friction decomposes the natural quantity of electricity which resides in the mineral.

XIV. *On the Specific Heat of the Gases.* By W. T. Haycraft, Esq.

(Read November 3. 1823.)

The experiments which I now submit to the Royal Society are repetitions of those I made many months ago, for the purpose of ascertaining the Specific Heats of the Gases. The importance of the subject so impressed my mind, that I determined to spare no pains in the prosecution of the inquiry, and therefore I willingly withheld my first experiments from the public eye, until, by a fresh series, I might present them with the greater confidence. The apparatus employed in these experiments was calculated to operate upon greater quantities of the Gases than the former one, and as every precaution which had been suggested was adopted, they have, perhaps, given even more decisive results than the last. The results themselves, however, are in every important particular exactly the same. It is also but justice to myself to state, that the conclusions which the former experiments led to, were exactly the reverse of what I had anticipated, and that they seemed at the time totally opposed to the doctrines of Black and Crawford, which I am still disposed to credit to a limited degree.

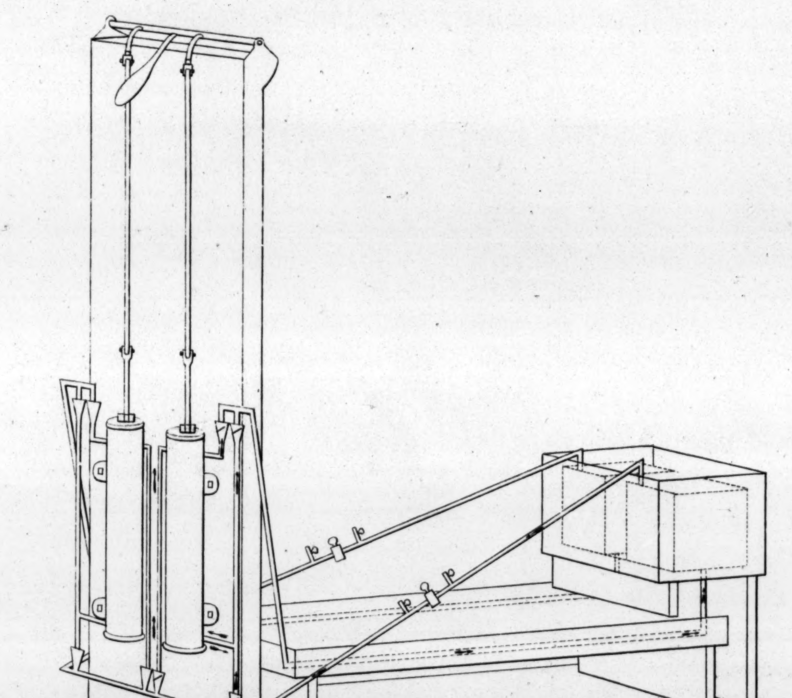
Before I enter into the detail, it will be necessary to take notice of the modes in which former experimenters have proceeded in these inquiries, and to point out what I conceive to have been the sources of fallacy in some of their conclusions. Of all these modes, none were more elegant than that adopted by Professor Leslie; but as he himself states, that their results were discordant with each other, it seems unnecessary to enter into a de-

scription of it. DR CRAWFORD's method consisted of inclosing two different Gases (previously exposed to muriate of lime, for the purpose of depriving them of their watery vapour) in two close vessels of equal size and weight; these being heated to exactly the same temperature, by a very ingenious contrivance, were at the same time plunged into two vessels, containing water of a lower temperature: these vessels were also of the same size, form, and weight: then, by means of accurately adjusted thermometers, he ascertained the comparative rise of temperature occasioned by the two Gases, and hence he determined their specific heats.

I know of no imperfection in this mode, excepting that the quantities of the Gases were so small, that the results could not be obtained with sufficient accuracy.

This defect is entirely obviated by the method adopted by Messrs DE LA ROCHE and BERARD: their apparatus consisted of a column of water, so adjusted as to act with a constant and equal pressure in a close vessel containing air; which being gradually expelled by the superincumbent water, pressed on the outward surface of a bladder containing the Gas whose capacity was to be examined. From this bladder the Gas was propelled through the calorimeter: this consisted of a vessel containing water of a low temperature, through which a spiral tube passed to conduct the Gas. Previous, however, to its entering the calorimeter, the Gas was heated, by a particular contrivance, to the boiling temperature. After leaving the calorimeter, it was conducted, by means of turn-cocks, into another bladder; the latter was acted upon in the same way as the former. By means of this reciprocating action, Messrs DE LA ROCHE and BERARD could cause 225.2 cubic inches of Gas, heated to the boiling temperature, to pass through the calorimeter every minute. The temperature communicated was ascertained by a thermometer, and from comparative trials, the capacity of the different Gases was inferred.

PLATE VIII.



This last method was superior to that of Dr Crawford, inasmuch as greater quantities of Gas could be employed. In other respects it was far inferior, because the experiments were not, strictly speaking, comparative. Atmospheric air, whose capacity was their standard of comparison, was subjected to trial, and the results were remarked. The other Gases, at different periods, with the surrounding media of different temperatures, and under different barometrical pressures, were examined; this plan involved endless and very difficult calculations, in order to adjust those differences. But the greatest imperfection in those experiments, was the neglect of depriving the Gases of their watery vapours previous to their examination. The apparatus itself would not admit of this, because the water employed in the process would necessarily keep the gas and the whole apparatus in a state of moisture. Besides, this very great source of error was materially increased by the high temperature to which the Gases were exposed, being a condition in which they are disposed to unite with a greater quantity of watery vapour than at ordinary temperatures. Considering the subject in this point of view, therefore, the experiments of Messrs De la Roche and Berard may be supposed to determine the capacities of the different Gases united with watery vapour at the boiling point, but by no means of those Gases in their dry state, and at ordinary temperatures.

The apparatus now to be described, will perhaps be found to unite the advantages, and avoid the defects of both methods.

It consisted of two hollow brass-cylinders, (see Plate VIII.), in each of which was a piston attached to a spindle by means of two levers of equal length; to the spindle was attached another lever, terminating in a handle, to be moved by an assistant. Each cylinder was closed at each end, excepting where the tubes were attached, which served to conduct the Gases. By means of four valves to each cylinder, fixed in such a way as, though difficult to

describe in writing, may be easily understood by reference to Plate VIII. *Each* action of the piston forced a quantity of air through the tubes; thus, by means of one additional valve, the apparatus would act upon exactly twice the quantity of air that could be acted upon in a pump of the ordinary construction.

The pipes immediately connected with the four valves, terminated in two tubes; through one of which the air, during the action of the apparatus, was propelled with a constant, and almost uniform current, while, through the other, the same air having passed through the heating apparatus and the calorimeter, returned to the cylinder, to be acted upon again in the same way. The heating apparatus just mentioned, consisted of a metallic vessel, about 16 inches long, containing hot water, through which the tubes passed, containing the air propelled from the cylinders: those tubes traversed the heating vessel three times before their exit, more effectually to secure the Gases arriving at the temperature of the water contained in the vessel. By means of a lamp placed under this vessel, I could raise the temperature of the water to any point required. This last arrangement, however, was rather a matter of convenience than necessity, as it will be easily perceived that, from the mode of conducting the experiments, a fixed point of temperature was not required.

There were also two calorimeters, similar in construction to those of Messrs De la Roche and Berard before described. Each of these was connected with the tube containing the Gas, propelled by its cylinders through the heating apparatus, and likewise with that through which the air flowed to the cylinder; these tubes were all of metal, and air-tight.

The apparatus, then, must be considered as consisting of two distinct parts, exactly the counter parts of each other, each conveying an equal quantity of Gas through the same heating medium, but through separate calorimeters.

The tubes communicating between the heating vessel and the calorimeters were one inch in length. In these tubes there was an opening, through which could be introduced a delicate thermometer, for the purpose of ascertaining the temperature of the Gases as they entered the calorimeters.

Each of the calorimeters was inclosed in a polished metallic case, for the purpose of preventing, as much as possible, the absorption or escape of caloric during the process. These latter were also placed in a box containing water, which was repeatedly agitated, that the calorimeters might not be affected by the unequal temperatures of the walls of the apartment.

For the purpose of facilitating the operation of filling the apparatus with the Gas operated upon, there was a turn-cock fixed in the course of each returning tube, by which the current of Gas through the tube was interrupted. Two smaller turn-cocks also were fixed in the same tube, one on each side of the larger turn-cocks: these, when open, communicated with the external atmosphere. When, therefore, the large turn-cock was closed, and the small ones open, the air would necessarily, during the action of the machine, rush in at one of the small turn-cocks, and be forced out of the other, so that the air contained in the apparatus would be constantly renewed. In order, then, to fill the machine with the Gas, nothing more was necessary than to form a connection between the gasometer or receiver containing the gas and the apparatus, by means of a tube connected to the small turn-cock first mentioned, through which the air rushed in. In performing this operation, however, I usually made use of an air-pump to exhaust the apparatus, and then opening the turn-cock communicating to the gasometer, filled it with the Gas required; after this operation had been several times repeated, I found the gas contained in the machine to be nearly as pure as that contained in the gasometer.

By a slight consideration of this description of the apparatus, which may be deemed rather prolix, and by an inspection of the plate, it will be perceived that two Gases, contained in the two parts of the machine, will be under circumstances precisely similar; the quantities of Gas transmitted through the calorimeters in a given time will be the same; the temperature of the surrounding media and the barometrical pressure will be equal; the temperature also of the Gases themselves must be the same, because they passed through the same heating medium. In fine, the size of the tubes, cylinders, calorimeters and valves, were the same in the two parts of the machine.

Therefore, the temperature communicated by the two Gases submitted to a comparative trial, will be the direct ratio of their comparative capacities for caloric, provided there be no disproportionate escape by absorption in the calorimeters, arising from the different temperatures of surrounding bodies.

This source of fallacy was obviated by the arrangement of Count RUMFORD, who contrived that the temperature of the surrounding medium should be as much above that of the calorimeter at the beginning, as it was lower at the end of the experiments.

The quantity of Gas propelled through the calorimeter was 12 cubic inches during the action of the piston. Those actions, as regulated by a second pendulum, which was suspended in the apartment, being 120 every minute, the whole quantity would be 1440 cubic inches of Gas propelled through the calorimeter every minute. There was no occasion, however, to take these quantities into the account, because they were precisely the same of each Gas subjected to trial.

My thermometers were adjusted by MR ADIE of Edinburgh. Each degree was divided into 5 parts, which were sufficiently large to be divided by the eye into 4 parts; so that the temperature could be ascertained to a 20th part of a degree, making al-

lowance for the imperfection of all instruments. Each calorimeter was furnished with its thermometer, the bulb of which was placed equidistant from its four sides: two smaller ones were placed so as to ascertain the temperature of the Gases before entering into, or coming out, of the calorimeter. One was attached to the heating vessel, and another to the vessel of water which served as the surrounding medium of the calorimeters.

Having filled both the calorimeters with water of the temperature of 42°, and the heating vessel with it at a temperature of about 180°, I admitted atmospheric air into each part of the apparatus. The pistons were put into motion, and continued till each of the calorimeters arrived at a temperature of 84°, with a variation of little more than one-twentieth part of a degree. Thus the temperature of the calorimeters was raised 42° each, with a correction of $\frac{1}{800}$th part of the whole. Much greater allowances may very properly be made for the imperfections of the instruments. This experiment was designed to prove the accuracy of the apparatus, and was often repeated, at different periods, with the same event. I was assisted in the following experiments by my friend Dr Clendinning, to whom I am much indebted for their success.

Experiments on Carbonic Acid.

No. 1.

The part of the apparatus which I call A was filled with carbonic acid, obtained from carbonate of lime; the part B with common air. In each of the cylinders was placed, in a proper receptacle, a quantity of very dry muriate of lime, for the purpose of perfectly freeing the Gases from watery vapour. The calorimeters being filled with water at a temperature of 42°, and the heating vessels with water at 149° $\frac{10}{22}$, the following results were obtained.

	Temperature of Calorimeter A, through which the Carbonic Acid passed.	Temperature of Calorimeter B, through which Atmospheric Air passed.	The comparative specific Heat of Carbonic Acid inferred from the comparative rise of the Temperature of the Air being 10000.
At the beginning of experiment,	42° Fahr.	42° Fahr.	
After 15 minutes,	$68\frac{2}{20}$	$68\frac{16}{20}$	9730

No. 2.

At the beginning of experiment,	$42\frac{1}{20}$	$42\frac{1}{20}$	
After 15 minutes,	$66\frac{10}{20}$	$66\frac{14}{20}$	9919

No. 3.

At the beginning of experiment,	42	42	
After 40 minutes,	$71\frac{10}{20}$	$71\frac{8}{20}$	10035

No. 4.

At the beginning of experiment,	45	45	
After 35 minutes,	$68\frac{5}{20}$	$68\frac{4}{20}$	10021

No. 5.

At the beginning of experiment,	$45\frac{14}{20}$	$45\frac{15}{20}$	
After 25 minutes,	$63\frac{5}{20}$	$63\frac{6}{20}$	10000

In these experiments it will be perceived, that the two first indicate that carbonic acid has a less capacity for caloric than common air. The three last, however, which do not differ materially from each other, will indicate an equal capacity, if we take the average of their results. The cause of the two first experiments indicating a lesser capacity, I suppose to arise from the gas not being perfectly freed from watery vapours. In the experiments I made last year, I observed that it was necessary to expose this Gas to the drying influence of muriate of lime, for 35 minutes at least, before it indicated the same specific heat as atmospheric air. This is not the case with all the other Gases: from hence I would infer, that it has a greater affinity with watery vapour.

The Gas contained in the gasometer, as indicated by limewater, contained 99 per cent. of carbonic acid; that taken from the apparatus after the experiments were concluded, by the same test, contained 90 per cent. The temperatures of the Gases while entering the calorimeters were equal, as indicated by the thermometers. It is worthy of remark, however, that these temperatures appeared several degrees lower than that of the water contained in the heating apparatus through which they passed. This will be easily explained, when we consider that a thermometer can never indicate the true temperature of any gas or vapour, which is itself pervious to the radiation of heat or cold from surrounding bodies. On this account the thermometers indicated a temperature of the Gases much lower than the true one, they being necessarily placed so near the calorimeters, which usually contained water of a temperature nearly 100° lower than that of the gases. In the same manner, the gases issuing from the calorimeters appeared to have a temperature something lower than that of the calorimeters themselves, being surrounded with objects of a lower temperature than that of the calorimeter.

Experiments on Oxygen Gas.

No. 1.

Having filled the part A with Oxyen Gas procured from the Black Oxide of Manganese, and every arrangement being made as before, the following results were observed:

	Temperature of Calorimeter A, containing Oxygen Gas.	Of Calorimeter B, containing Atmospheric Air.	Inferred capacities.
At the beginning of experiment,	$45^\circ\frac{6}{20}$	$45^\circ\frac{5}{20}$	
After 5 minutes,	$61\frac{16}{20}$	$61\frac{15}{20}$	10000
After 10 minutes,	$67\frac{2}{20}$	$67\frac{1}{20}$	10000
After 15 minutes,	71	$70\frac{18}{20}$	10019
After 20 minutes,	$74\frac{9}{20}$	$74\frac{9}{20}$	9982

No. 2.

	A	B	
At the beginning of experiment,	56.6°	56.4°	10000
In 10 minutes,	66.16	66.14	10000
In 15 minutes,	71	70.18	10000
In 20 minutes,	$74\frac{4}{20}$	$74\frac{2}{20}$	10000

The temperature of the Gases entering the calorimeters were equal, being each 137°. The Gas contained in the gasometer before the apparatus was filled, indicated 98° per cent. of Oxygen, by the test of sulphuret of lime. After the experiment was concluded, that contained in the apparatus indicated 91° per cent.

Experiments on Hydrogen.

Hydrogen Gas was procured from the decomposition of water by means of sulphuric acid and zinc. The part B was filled with the same, and the following experiments were made.

No. 1.

In this experiment the calorimeters were filled with water of the same temperature, and the process was conducted on rather a different principle than the former, namely, it was continued until the calorimeters ceased to rise in temperature, or rather, till the temperature began to fall. This latter circumstance would take place when the heat communicated by the Gas was exactly equal to that abstracted by the colder surrounding medium. The number of degrees of temperature, then, which each Gas would sustain in its calorimeter, will be the ratio of its power for giving out heat, and consequently of its capacity for caloric.

The temperature of calorimeter A, at the beginning of the experiment, was about 50°, and after 105 minutes, the temperature of calorimeter A was $82^{\circ}\frac{15}{20}$, and that of B, containing Hydrogen Gas, was $82^{\circ}\frac{10}{20}$, and the surrounding medium $60^{\circ}\frac{9}{20}$, indicating the comparative capacity of Hydrogen to be 98.64, being a difference so trifling, that it may be regarded as the same as that of atmospheric air; if we make allowance for the evident greater ratio in its heating, and the smaller ratio of its rate of cooling at the end of the experiment. This will be seen by the following Table.

	Temperature of A, containing Atmospheric Air.	Temperature of B, containing Hydrogen Gas.
At the beginning of experiment,	50°	50°
In 5 minutes,	59	58.6
In 10 minutes,	$67\frac{16}{20}$	$66\frac{14}{20}$
In 15 minutes,	$71\frac{16}{20}$	$70\frac{4}{20}$
In 20 minutes,	75	$73\frac{4}{20}$
In 25 minutes,	$77\frac{16}{20}$	76
In 30 minutes,	79	$77\frac{6}{20}$
In 31 minutes,	$80\frac{12}{20}$	$78\frac{10}{20}$
In 40 minutes,	$81\frac{12}{20}$	$80\frac{3}{20}$
In 45 minutes,	$82\frac{8}{20}$	81
In 50 minutes,	83	$82\frac{9}{21}$
In 55 minutes,	$83\frac{1}{20}$	$82\frac{8}{20}$
In 60 minutes,	$83\frac{2}{20}$	$82\frac{12}{20}$
In 65 minutes,	$83\frac{3}{20}$	$82\frac{16}{20}$
In 70 minutes,	$82\frac{16}{20}$	$82\frac{10}{20}$

No. 2.

			Inferred capacity.
At the beginning of experiment,	$49\frac{8}{20}$	$49\frac{7}{20}$	
After 5 minutes,	$55\frac{6}{20}$	$55\frac{10}{20}$	10500
After 10 minutes,	60	$60\frac{8}{20}$	10424
After 15 minutes,	$64\frac{10}{20}$	$64\frac{8}{20}$	9950
After 20 minutes,	$67\frac{2}{20}$	$67\frac{2}{20}$	10002
After 25 minutes,	$69\frac{4}{20}$	$69\frac{3}{20}$	10000

This last experiment was conducted as the former ones.

The air appeared, after the experiments, to contain 88 per cent. of Hydrogen Gas, as indicated by explosion with Oxygen Gas *.

In these two experiments it may be observed, that the watery vapour which may be presumed to be in the Hydrogen Gas, before it had been sufficiently exposed to the drying influence of the muriate of lime, seemed to decrease in specific heat, exactly contrary to what might be expected. In the first experiment, at the expiration of the first five minutes, it had a capacity of 9222, pretty nearly the same as indicated in the experiments of Messrs DE LA ROCHE and BERARD ; but in proportion as the experiment had advanced, and the hydrogen had been exposed longer to the muriate of lime, its specific heat approached to that of atmospheric air, till, at the end of the experiment, they were quite equal.

No. 2. was performed upon the same hydrogen, in its driest state; and throughout the whole experiment it indicated also a capacity equal to the standard. In this experiment I know of no source of fallacy, as the Gases, while entering into the calorimeters, were of exactly the same temperature, and care was taken to ensure accuracy.

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* The apparatus which I found most convenient for exploding gases, is a modification of Dr URE's syphon eudiometer. It consists of a hole bored in the solid bottom of a mercurial trough, representing an inverted syphon; one end of which opens into the part containing mercury, and the other through the edge of the trough to the open air. To the latter opening is cemented an open glass tube; and to the former a common graduated eudiometer is made to fit accurately. When this apparatus is used, the graduated tube is filled in the usual way, and applied to the opening communicating with the trough. Mercury is poured into the other tube, to the same height as that contained in the graduated one. The finger is then applied to the open tube, and the electric spark passed. After the explosion, more mercury is poured into the open tube, to the same height that it had risen in the eudiometer, after which the degrees are read off.

Azote.

Of Azote I shall merely state, that, last year, I performed similar experiments upon this Gas, the results of which were perfectly analogous with those now detailed; and as all the experiments agree that it has by volume the same specific heat as atmospheric air, namely 1000, I thought it needless to repeat them.

Carburetted Hydrogen.

In my former experiments on Carburetted Hydrogen, procured from the decomposition of sea-coal, I concluded that it also had the same capacity as atmospheric air; but I have since found that the capacity of this Gas varies extremely, according to the modes in which it is procured. That produced from sea-coal seems to have a capacity nearly equal to the standard; that from the decomposition by heat of animal fat, has a much greater capacity. From the following experiments, however, it will appear that olefiant Gas owes its increased capacity to the empyreumatic or ethereal vapour with which it is usually combined.

No. 1.

This experiment I conducted in the same way as No. 1. on Hydrogen Gas. The part B was filled with olefiant Gas, obtained from the gas-pipes of a public company. The calorimeters at the beginning of the experiment contained water of the temperature of 50°. At the end of 50 minutes the calorimeter A had acquired its utmost temperature of $92°\frac{7}{20}$, and of B that of $93°\frac{12}{20}$; the surrounding medium being $66°\frac{8}{20}$.

No. 2.

The calorimeters were of a temperature of $52^\circ\frac{5}{20}$ at the beginning of the experiment: after 55 minutes, the calorimeter A had acquired a temperature of $92^\circ\frac{10}{20}$, and B that of $94^\circ\frac{4}{20}$; the surrounding medium being 65°. The average result of these experiments, Nos. 1. and 2., indicates the specific heat of olefiant gas to be 10559. Though the results of these two experiments do not quite agree with those I formerly made, yet the difference is very trifling, and may be supposed to arise from the greater freedom of the gas I formerly made use of, from empyreumatic vapour. This will appear probable from the following experiments.

No. 3.

The part of the apparatus B was filled with carburetted hydrogen, procured by the destructive distillation of mutton-suet. The calorimeters were filled with water of the temperature of $50^\circ\frac{15}{20}$. At the end of 40 minutes, the calorimeter, through which the olefiant gas passed, had acquired its extreme temperature of 95°, the other that of $88^\circ\frac{10}{20}$; the surrounding medium being $65^\circ\frac{2}{20}$; indicating the specific heat of olefiant gas to be 12777.

That the gas procured from animal fat contains more empyreumatic vapour, is evident from its sensible qualities, which may account for its greater specific heat, compared with that procured from sea-coal. The gases, at the end of the experiment, were exactly of the same temperature as when entering into the calorimeters.

No. 4.

The last experiment was repeated, except that the olefiant gas was procured from alcohol and sulphuric acid. After 25

minutes, the calorimeter A had assumed the temperature of $74\frac{4}{20}°$, and calorimeter B that of 75°.10; the surrounding medium being 54°; indicating the capacity of olefiant Gas to be 10643.

No. 5.

The last experiment was repeated, and gave a result of 10674; the medium result of experiments Nos 4. and 5. being 10658, indicating the capacity of olefiant Gas procured from alcohol and from sea-coal to be almost exactly the same.

No. 6.

Wishing to ascertain if the ethereal or empyreumatic vapour in olefiant Gas affected its specific heat, I poured a few drops of sulphuric ether into the part of the apparatus containing atmospheric air, that the latter, as well as the olefiant Gas, being equally saturated with the vapours of ether, it might be ascertained what effect that condition might have on the capacities of the Gases. The part B contained the olefiant Gas as before. After 40 minutes, both the calorimeters had acquired a temperature of $85\frac{3}{20}°$, the surrounding medium being $61\frac{4}{20}$. The inference, then, may fairly be made, that it is the combined vapour that increases the specific heat of olefiant Gas.

Experiments on the Air of Respiration.

Having last year made more than ten experiments which prove that the mixtures of carbonic acid with atmospheric air exposed freely to water, and at a temperature of about 100°, had a much less capacity for heat than atmospheric air had, under ordinary circumstances, and this curious fact seeming to throw some light upon the physiology of animal respiration, I filled the

part B with air from the lungs, and the part A with atmospherical air.

The heating apparatus was kept, by means of a lamp, at the temperature of between $97°\frac{10}{20}$ and $100°\frac{10}{20}$. After the end of 35 minutes, the calorimeter, through which the air of respiration passed, attained the temperature of $59°\frac{4}{20}$, and the other that of $61°\frac{4}{20}$; the surrounding medium being 54°.16, indicating the air of respiration to be 6875.

No. 2.

The last experiment was repeated, when the calorimeter arose from $56°\frac{2}{20}$ to $58°\frac{10}{20}$, and B from $56°\frac{3}{20}$ to $57°\frac{16}{20}$, indicating the capacity of the air of respiration to be, as in the last experiment, 6875.

It may not be improper in this place to state, that, in my former experiments, mixtures of carbonic acid and atmospheric air, under different conditions of temperature, and combination with watery vapour, had relative capacities of 3333,66666.9999, and 13333. It was my intention to have repeated those experiments in such a way as to ascertain the precise conditions under which these changes of capacities took place; but, from various engagements, I am unable to do so. I may remark, however, that the last two experiments seem to indicate, that the air of respiration enters into the second of this series, making allowance for the difference of the standard of comparison; this being, in my former experiments, common undried atmospherical air, while the standard of the latter was the same air carefully dried.

There is also a curious coincidence between this last-mentioned series of capacities of gas in different states of combination with water, and the expansive forces of air combined also with different proportions of watery vapours. Having procured a glass globe, to which a small stem was connected, in such a way

that mercury contained in the hollow ball would rise into the stem upon any increase of the expansive force of the air contained in the ball, I filled the latter with air at a temperature of 60° ; after which the ball was immersed into boiling water. In a short time the mercury rose into the stem to the height of 7 inches. The experiment was repeated, excepting that a few drops of water were put, together with the air, into the ball. The mercury, after the immersion of the ball in boiling water, rose to 21 inches. Afterwards, on passing a quantity of water into the ball, the mercury, after its immersion, rose to 28 inches. Some months afterwards, on repeating the experiment, the mercury rose in one instance to 14 inches. Thus we have a series of expansive forces of air united to watery vapour of 7, 14, 21 and 28 inches : it was upon this principle that I contrived an air thermometer. The form of it is similar to that of the differential thermometer invented by Professor Leslie. One ball contained atmospherical air dried by means of muriate of lime; the other contained air in its usual state. Interposed between the balls was a column of the volatile spirit of turpentine. Upon any rise of the temperature of the atmosphere, the column immediately rose at the side of the dry ball. After some time, however, the instrument seemed to have lost its power; and after a still longer period, the ball containing dry air had the greater expansive force. This I accounted for by supposing, that the vapour of turpentine had in process of time combined with the dry air, and had given it its greater expansive power. This thermometer is now a remarkably delicate one, though its degrees are of very unequal length, and appear to vary by lapse of time. Probably hydrogen gas contained in two platina balls, in one of which a little mercury might be placed, connected together in the same way, would make an accurate pyrometer, indicating temperatures as high as the melting point of platina.

There is another condition under which air is capable of a great variety of specific heats, namely, when it exists in different degrees of density, whether arising from pressure or other causes. The increased capacity of air, when under lesser degrees of atmospheric pressure, has been properly made use of to explain the extreme cold which exists in high regions; and its decreased capacity under mechanical pressure, also satisfactorily accounts for the heat evolved under that condition. This principle, so far as I know, has not been used to explain one cause of the intense heat produced during the combustion of gunpowder and other explosive mixtures. If we reflect a moment, however, we shall perceive that the resistance of the pressure of the atmosphere to the expansion of the nascent gases produced by the combustion, will cause them to exist in a state of greater density than when the resistance of the atmosphere has been finally overcome. It is during this state of potential compression, if I may use teh term, that the intense heat is produced. After the first explosion, however, the gaseous products will expand, and then there will necessarily be an absorption of caloric, and consequently comparative coldness, produced. In order to ascertain whether there is a permanent evolution of caloric, occasioned by the combustion of gunpowder, I made the following experiment.

Having a receiver containing 528 cubic inches, filled with water of a temperature of 52°, placed in a pneumatic trough, the surrounding atmosphere being also 52°, I introduced 240 inches of the aëriform fluids, produced during the combustion of that composition of gunpowder which is used for pyrotechnical purposes. After the explosion, the gas in the upper part of the receiver had acquired a temperature of nearly 54°, and the water not so much. This experiment shews, that though heat is evolved in the combustion of gunpowder, its quantity is not nearly so great as has been imagined. Again, if we consider that the products of the combustion of gunpowder have not, by direct experiment,

been proved to have a greater specific heat than the ingredients of that composition, the phenomenon of heat being produced during that combustion should not be urged as an objection to the hypothesis of BLACK and CRAWFORD. Indeed it appears very probable, from the inspection of the Table of Specific Heats of Different Bodies, that those elastic products have a less capacity than the ingredients of gunpowder, from which they are produced. For example, azote, which composes two-thirds of the elastic products, has a capacity of 2669, and carbonic acid, comprising one-third of the products, if my experiments are to be trusted to, has a capacity of only 1751, water being 10000. Nitric acid of a specific gravity of 1,1354, has a capacity of 5760. The azote, therefore, and oxygen, which is produced from the decomposition of one of the ingredients forming the elastic products of not half the specific heat of that ingredient, should, according to the hypothesis of BLACK, evolve heat. This might take place even if we make allowance for the lesser capacity which nitric acid has in its state of one of the ingredients of the nitrate of potash.

The same condition of potential compression may also contribute to the intense heat which takes place in a blast-furnace. This heat is known by all conversant with the phenomenon to be, not in a ratio of the fuel consumed, but of some compound ratio. This may be explained in the following manner: 1st, A quantity of air is forced into contact with the coals in a state of ignition, and its temperature is suddenly raised extremely high. 2d, In this condition, were it not for the pressure of the atmosphere, it would become as suddenly expanded. 3d, Had this expansion taken place, it would have acquired an increased capacity, and would consequently have absorbed a considerable portion of the caloric evolved by the combustion, tending thereby to lessen the capacity of the heat. 4th, But the heated air being prevented by the pressure of the atmosphere from expand-

ing in a ratio equal to the temperature acquired, the absorption of caloric is lessened, and a greater proportion of the heat of combustion is rendered free. Thus, although the total quantity of caloric evolved at, and consequently to combustion, may be in a direct ratio of the quantity of fuel consumed; yet the intensity of the thermometrical heat at the moment, and at the place of combustion, will be greater in a compound ratio, directly as the pressure of the atmosphere, and inversely as the times of expansion of the air employed in the blast. These times are, of course, inversely as the intensity of the blast. The thermometrical heat, then, at the moment and place of combustion, will be in a compound ratio of the quantities of fuel consumed, the weight of the atmosphere, and the quantity of air employed in the blast in a given time. The same rule will hold even in what are called Chimney Furnaces; and it is ascertained by experience, that those furnaces of steam-engines through which a greater quantity of air passes in a given time, consume a proportionally less quantity of fuel to produce the same effect. Probably blast-furnaces might be advantageously employed in lessening the quantity of fuel used for those valuable machines.

Although, according to the foregoing experiment, it appears contrary to my original expectation, that, by volume, oxygen gas has the same specific heat as carbonic acid, it by no means follows that caloric should not be evolved during the formation of the latter by combustion. This formation does not consist of a conversion of oxygen into carbonic acid, but of a union of two ingredients into a compound, having an absolute capacity for caloric equal to one of the ingredients only, namely, the oxygen gas; consequently the whole absolute heat of the carbon is rendered free.

The direct results of these experiments shew, that the specific heats of all the Gases experimented upon, are to each other

inversely as their specific gravities; and, 2dly, That different states of combinations of the Gases with aqueous and other vapours, affect the capacities of the Gases, and that probably, in some instances, in a regular arithmetical progression, corresponding with the arithmetical rate of expansive force of the Gases in different states of combination with vapour. The most interesting result to the physiologist is, that the air of respiration, at a temperature of between $100\frac{1}{2}°$ and 95°, has a less specific heat than atmospherical air. Many experiments were made which are not here detailed, which shewed that the air of respiration, at the temperature of 102° and upwards, and of 91° and downwards, had a capacity the same as that of atmospherical air. I should feel a hesitation in stating these results, had not experiments, very often repeated, during a course of several months, warranted me in my conclusions.

PLATE IX.

Fig. 1.

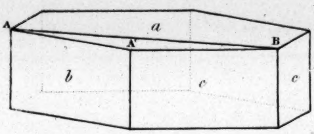


Fig. 2.

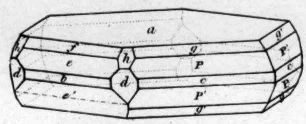


Fig. 3.

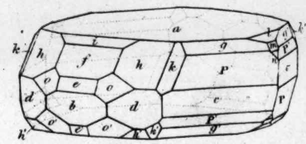


Fig. 4.

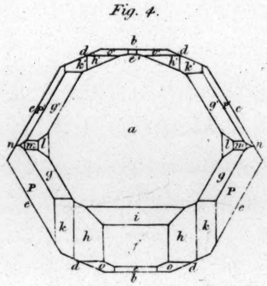


Fig. 5.

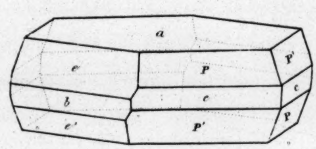


Fig. 6.

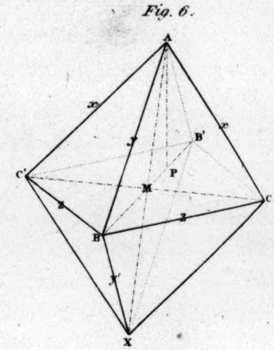


Fig. 7.

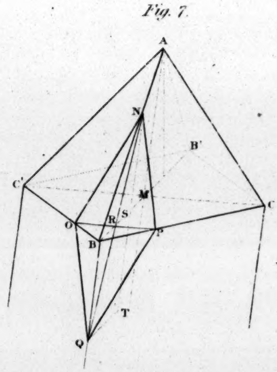


Fig. 8.

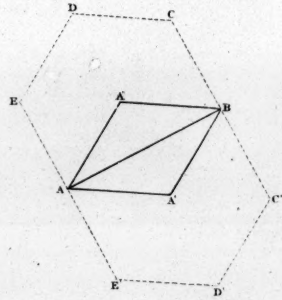


Fig. 9.

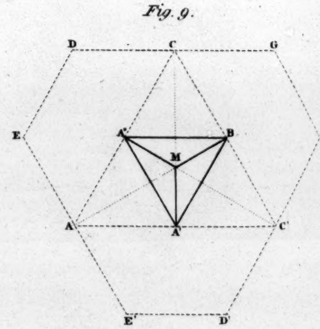


Fig. 10.

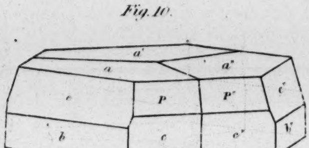


Fig. 11.

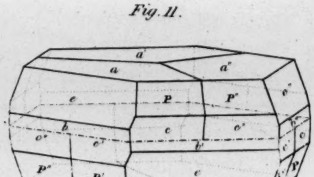
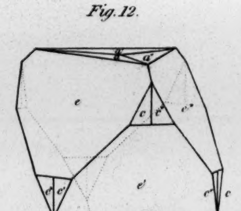


Fig. 12.



W. Miller Sculp^t.

XV. *On the Forms of Crystallisation of the Mineral called the Sulphato-tri-Carbonate of Lead.* By W. Haidinger, Esq. F. R. S. E.

(Read February 16. 1824.)

The history of the progress of discovery in the Natural Sciences, records innumerable instances, where the different ways of considering the same subject have created apparent contradictions, which have subsequently been reconciled by more accurate examination. The first observation of facts is very often far from being accurate; and, in most cases, this inaccuracy cannot be corrected, till the science itself has attained a higher degree of perfection. The optical and crystallographic inquiries into the nature of mineral substances, refer equally to the regular forms which these bodies present. The object of Crystallography is to ascertain this regular form from direct observations; and that of the department of Optics, which relates to this subject, is to determine the action of regularly crystallised bodies upon light. According to certain general laws, it has been found possible to argue from the optical phenomena to the external forms of minerals, and inversely from these forms to the actions dependent upon them, which affect light in its passage through crystallised substances.

It may be considered as a general law, as has been shewn by Dr Brewster, that the external form of a crystallised body, exhibiting two axes of no polarisation uniformly throughout its whole substance, cannot belong to the tessular, to the rhombohedral, or to the pyramidal system. The observations of crystallographers upon a few species, seem still to stand in opposition

to this law, and to indicate certain exceptions to its generality; but as the existence of two polarising axes in a mineral, whenever it is capable of being observed at all, can always be ascertained in the most unequivocal manner, and as several of the crystallographic observations date from an early period of the science, these exceptions are less likely to arise from the want of correctness or generality in the law, than from a want of precision in observing the properties of minerals; and hence a careful re-examination of them is a subject deserving the particular attention of crystallographers. The Sulphato-tri-Carbonate of Lead from Leadhills, as hitherto described, constitutes one of these exceptions. It is the object of the present paper to remove the difficulties in respect to this species, by a more accurate investigation of its crystalline forms.

Count Bournon seems to have been the first author who erected this substance into a separate species, under the name of *Plomb Carbonaté Rhomboidal*, which he describes * as presenting the form of a regular six-sided prism, or that of an acute rhombohedron of 70° 32′ (the plane angles being given = 60° and 120°), variously modified by planes perpendicular and parallel to its axis. He considers the acute rhombohedron as the primitive form of the species. Mr Brooke †, who calls this substance the *Sulphato-tri-Carbonate of Lead*, likewise states the form of the crystals most commonly occurring, to be a regular six-sided prism, or an acute rhombohedron of 72° 30′, terminated by a plane perpendicular to its axis; the latter being parallel to the perfect planes of cleavage. He mentions, besides, a considerable number of secondary faces, and he has given the drawing of a variety, which contains them, in the third edition of Phillips's Mineralogy, page 342., where he likewise assumes the acute rhombohedron as the primary form of the mineral. Supposing the angles given by Mr Brooke to be exact, Professor

* Catalogue de la Collection Minéralogique, p. 343.

† Edinburgh Philosophical Journal, vol. iii. p. 118.

Mohs also considered it as rhombohedral, and arranged it in the genus Lead-baryte, under the denomination of the *Axotomous Lead-baryte*, being most distinctly cleavable in a single direction intersecting the principal axis. Contrary to the general observation, that rhombohedral substances possess only one axis of no polarisation, Dr Brewster * found that the mineral in question exhibited two systems of coloured rings, more distant from each other than those of Carbonate of Lead; and from the existence of the two axes, he inferred that its forms belonged to the prismatic system. He also remarked, that many crystals contain films oppositely crystallised, as is the case in Arragonite.

I had at various times attempted to examine the forms of Axotomous Lead-baryte, without, however, attaining a sufficiently correct result; but having lately resumed this examination, the beautiful specimens in Mr Allan's collection, and several others, equally interesting, with which I had been favoured by Dr Brewster, Mr Irving, and Mr T. Dowler, have enabled me to ascertain the forms of this species with a considerable degree of accuracy. The results of this examination are very remarkable. They exclude the rhombohedron and the regular six-sided prism from the range of forms, which the individuals of the species may assume, and thus perfectly confirm the inference drawn by Dr Brewster from his optical observations, while they are at variance with the crystallographic statements both of Count Bournon and of Mr Brooke.

The crystals of Axotomous Lead-baryte have been described as regular six-sided prisms, having angles of 120°, and terminated by planes perpendicular to their axis. Upon examining them, however, by the reflective goniometer, I found that neither of these statements was correct, but that the six-sided prism is a combination of three different simple forms, *a*, *b*, and *c*, Fig. 1. Plate IX., whose angles of intersection differ more or less from

* Edinburgh Philosophical Journal, vols. iii. p. 138.; vi. p. 183.; ix. p. 367.

120° and 90°. The inclination of *a* upon *b* I found to be = 90° 29′; that of *c* upon *c* = 120° 20′; and that of *b* upon *c* = 119° 50′. Though slight, these differences are easily ascertained, and their consequences, in the disposition of the crystalline faces, are so obvious, that they would certainly not have escaped the practised eye of the crystallographers who described them, had not a particular mode of regular composition seemed to establish a kind of symmetry round a rhombohedral axis supposed to be perpendicular to the faces of cleavage.

The system of crystallisation to which the forms of Axotomous Lead-baryte belong, is not, therefore, the rhombohedral system, nor do these forms enter into that class of prismatic forms which exhibit the full number of faces of every simple form in the combinations, but they must be considered as hemiprismatic, the axis of crystallisation which is parallel to the edges of the prism *c*, being inclined to the base at an angle of 90° 29′.

There are two observations which can be very easily instituted on almost every group of well-pronounced crystals of the species, and which evidently prove that the forms of these are really hemiprismatic. The first of them refers to *oblique* truncations of the lateral edges, between *b* and *c*, as *d*,*d*, in Fig. 2., which are inclined to *b* at an angle of 156° 27′, and to *c* at an angle of 143° 23′. The other refers to the slightly, but very distinctly marked planes of junction, between two individuals in the regular compositions, in a direction joining alternating angles, like A and B, Fig. 1., in the supposed regular six-sided prism. The remainder, ABA′, of the terminal face of one of the individuals, is inclined to the similarly situated face of the other, at an angle of 179° 10′, to which, on the opposite side, corresponds a re-entering angle of 180° 50′. Both these facts I had observed separately in numerous specimens, but the smallness of well-pronounced crystals, and the impossibility of distinguishing by the eye an angle of 119° 50′, from one of 120° 20′, rendered it very difficult to combine these different observations into one representa-

tion of its forms. The observation, that the plane of the resultant optical axes passes through a line parallel to A′A, Fig. 1., led me to inquire whether the face *b*, which I had before supposed to be one of the faces of an oblique-angular four-sided prism, might not be parallel to the short diagonal of the prism produced by the enlargement of the faces *c*, *c*, &c. The examination of a small, but beautiful, and very regularly formed twin-crystal, in the collection of Mr Allan, carried on upon this supposition, gave at last the results which form the substance of this paper. One of the individuals of that regular composition is represented in Fig. 3.; and Fig. 4. is its projection, on a plane parallel to the face *a*, the whole of the crystal having been duly completed.

The number of faces which constitute this crystalline form is so great, and the form itself is, on that account, so complicated at first sight, that it will be more convenient to begin with considering some simple varieties of the substance, in order to afford a more distinct idea of its Series of Crystallisation.

One of these varieties is represented in Fig. 5. Beside the faces of Fig. 1., all those edges which are not parallel to the axis are replaced by the inclined faces *e*, *e′*, *P*, and *P′*. If enlarged to their mutual intersection, *P* and *P′* produce the fundamental form of the species, Fig. 6., a scalene four-sided pyramid, in which AX, the real axis of the form, is inclined to AP, a line perpendicular to the base BCB′C′, at an angle of 0° 29′. In the method of Professor Mohs the angle MAP is called the *Inclination of the Axis*. This method of considering hemiprismatic forms, is best calculated to render more striking those analogies which exist between the series of crystallisation of the species in which the axis is perpendicular, and of those in which it is inclined to the base of the fundamental form. The development of the formulæ expressing the angles of this kind of pyramids, depends upon the comparison between the lines AP, MB, MC, and MP.

If we designate AP by a, MB by b, MC by c, MP by d, and, moreover, call y and y' the terminal edges AB and AB′ contiguous to b, x the terminal edge AC, contiguous to c, and z the lateral edge BC, which joins the two diagonals of the base, b and c with each other, we obtain the following formulæ:

$$\cos y = \frac{a^2(b^2-c^2)-c^2(b+d)^2}{a^2(b^2+c^2)+c^2(b+d)^2};$$

$$\cos y' = \frac{a^2(b^2-c^2)-c^2(b-d)^2}{a^2(b^2+c^2)+c^2(b-d)^2};$$

$$\cos x = \frac{a^2(c^2-b^2)-c^2(b^2-d^2)}{\sqrt{[(a^2(b^2+c^2)+(b+d)^2c^2)(a^2(b^2+c^2)+(b-d)^2c^2)]}};$$

$$\cos z = \frac{b^2(c^2-a^2)-c^2(a^2+d^2)}{\sqrt{[(a^2(b^2+c^2)+(b+d)^2c^2)(a^2(b^2+c^2)+(b-d)^2c^2)]}};$$

$$\text{tang MAP} = \frac{d}{a};$$

$$\text{tang BAP} = \frac{b+d}{a};$$

$$\text{tang B'AP} = \frac{b-d}{a};$$

$$\cos \text{CAC}' = \frac{a^2+d^2-c^2}{a^2+d^2+c^2};$$

$$\cos \text{CBC}' = \frac{b^2-c^2}{b^2+c^2}.$$

The ratio of the lines $a:b:c:d$, which gave a result agreeing nearest with observation, was that of 120 : 95 : 54.5 : 1. The values of $a=120$, $b=95$, $c=54.5$, and $d=1$, being substituted in the above-mentioned formulæ, give the dimensions of the fundamental form as follows:

$$P = \begin{Bmatrix} 72^\circ\ 36' \\ 72\ \ 10 \end{Bmatrix};\ 124^\circ\ 50';\ 137^\circ\ 0'.$$

Moreover, we have the angle of inclination MAP $= 0^\circ\ 29'$; BAP $= 38^\circ\ 40'$; B′AP $= 38^\circ\ 4'$; CAC′ $= 49^\circ\ 51'$; CBC′ $= 59^\circ\ 40'$.

According to the method of crystallographic designation of Mohs, the faces *P*, *P*, next the observer, and contiguous to the upper apex of the fundamental form, are denoted by $\frac{P}{2}$, and the other faces *P′ P′*, contiguous to the same apex, are denoted by $-\frac{P}{2}$. If we suppose the axis of a series of such pyramids of equal bases, to increase and decrease according to the powers of the number 2, the limits at which this series will arrive are, on one side, a plane figure parallel to the base, on the other a four-sided prism parallel to the axis of the fundamental form. The face *a*, in the combination Fig. 5, corresponds to the first, and the faces *cc* to the second of these limits; while their crystallographic signs will be $P - \infty$ and $P + \infty$. The inclination of the axis being so slight, the difference between the angles of the base of P and those of the transverse section of $P + \infty$ does not amount to $0° \ 1'$; $P + \infty$ is therefore $= 59° \ 40'$.

Supposing the faces *a* and *b* to disappear from the combinations, the faces *e* will assume the figure of a rhomb NOQP, Fig. 7, at the solid angle of combination between $\frac{P}{2}$ and $P + \infty$. As the diagonals of any rhomb bisect each other; NR will be $=$ RQ. Draw NT parallel to BQ, and QT parallel to BM. Since the angle NRS is $=$ BRQ, and RSN $=$ RBQ, and the line NR $=$ RQ, the triangles NRS and QRB will be equal and similar; therefore NS $=$ BQ, and also $=$ ST, which will make NT $= 2 .$ NS. If a tangent plane, laid on the terminal edge AB of the fundamental pyramid produces the horizontal prism $P\breve{r}$, that plane, the corresponding axis of which is double the axis of the former, will produce a prism belonging to $P + 1$, and *e*, therefore, receives the crystallographic sign $\frac{P\breve{r}+1}{2}$. The inclination of *e* to *a* is $= 112° \ 0'$. As the situation of *e′* is analogous to that of *e*, its crystallographic sign is $-\frac{P\breve{r}+1}{2}$, and the inclination of *e′* to *a* $= 111° \ 11'$. Since the edges of combination between *e* and *b* are

horizontal, the form to which the latter faces belong is the limit of the series of horizontal prisms, contiguous to the long diagonal, and is consequently designated by $\mathrm{P\breve{r}} + \infty$. The sign of the whole combination is

$$\underset{a}{\mathrm{P}-\infty} \,.\, \underset{P}{\frac{\mathrm{P}}{2}} \,.\, \underset{e}{\frac{\mathrm{P\breve{r}}+1}{2}} \,.\, -\underset{P'}{\frac{\mathrm{P}}{2}} \,.\, -\underset{e'}{\frac{\mathrm{P\breve{r}}+1}{2}} \,.\, \underset{c}{\mathrm{P}+\infty} \,.\, \underset{b}{\mathrm{P\breve{r}}+\infty}.$$

Another combination is represented in Fig. 2. Besides those of the preceding variety, it contains the faces *d*, *f*, *g*, *g'*, and *h*. The faces *d* form oblique truncations of the edges between *b* and *c*, and they are inclined to the former at an angle of 156° 27′; if enlarged to their intersection above these faces, they will meet under an angle of 132° 54′, while that of the faces *c* and *c* is = 59° 40′. The diagonal *c* being supposed equal in both prisms, *b'* of the prism *d* will be $\frac{1}{4}$ *b* of the prism *c*. The edges of combination between *a* and *h* are parallel to those between *h* and *d*; the transverse section of the scalene four-sided pyramid produced by the enlargement of the faces *h*, is therefore similar to that of the prism produced by the faces *d*. But the edges of combination between *P* and *h* are parallel to those between *h* and *f*, *f* and *h*, and *h* and *P*, and consequently to the acute terminal edges of P; the ratio of *a'* and *b'*, or of the axis, and that diagonal of the base which corresponds to the long diagonal of P, will be in the derived pyramid the same as that of *a* and *b* in the fundamental form. The ratio of the three lines $a' : b' : c'$ of the pyramid *h* may be expressed by $a : b : 4\,c$, which is equal to $\frac{4a}{4} : b : 4\,c$. In the method of crystallographic designation of MOHS, the sign $(\mathrm{\breve{P}}')^m$ is given to a derived pyramid, one of the diagonals of which is equal to the similarly situated long diagonal of the fundamental pyramid P′, while the other diagonal and the axis are equal to *m* times the analogous lines in that form. In the case

under consideration m is $= 4$; but the axis of P′ is equal to one-fourth of the axis of P; the former pyramid consequently $=$ P $-$ 2, and the sign of the derived form becomes $= \frac{(\mathring{P}-2)^4}{2}$, the divisor 2 being added, because the pyramid occurs in the crystal only at that side of the axis which corresponds to $\frac{P}{2}$. The angle formed by the intersection of h and h is $= 142° 26'$. The prism d being the limit on one side of that series to which $\frac{(\mathring{P}-2)^4}{2}$ belongs, will be represented by $(\mathring{P}+\infty)^4$. It is evident that f, which appears with parallel edges of combination in the place of the terminal edge AB of P, must on that account be $= \frac{P\mathring{r}}{2}$. The inclination of this face to P $-\infty$ or a is $= 128° 40'$. From immediate measurement, and from their situation between a and P or P', the faces g and g belong to $\frac{P-1}{2}$, and g' and g' to $-\frac{P-1}{2}$. The edge produced by the intersection of g and g is $= 94° 18'$; and that produced by g' and $g' = 93° 52'$.

The inclination of	c to	a is	$=$	90° 14′;
	P	a	$=$	111° 42′;
	P'	a	$=$	111° 18′;
	g	a	$=$	128° 23′;
	g'	a	$=$	128° 5′.

The representation of the whole form by crystallographic signs is:

$$\underset{a}{P-\infty} . \underset{f}{\frac{P\mathring{r}}{2}} . \underset{e}{\frac{P\mathring{r}+1}{2}} . \underset{g}{\frac{P-1}{2}} . \underset{h}{\frac{(\mathring{P}-2)^4}{2}} . \underset{P}{\frac{P}{2}} . -\underset{e'}{\frac{P\mathring{r}+1}{2}} . -\underset{g'}{\frac{P-1}{2}} . -\underset{P'}{\frac{P}{2}} .$$

$$\underset{c}{P+\infty} . \underset{d}{(\mathring{P}+\infty)^4} . \underset{b}{P\mathring{r}+\infty} .$$

It will now be easy to determine the relation of the simple forms which enter into the combination Fig. 3.; those under which it is principally contained, being known from the preceding varieties.

The face *i* is determined, by immediate measurement, to belong to the horizontal prism, which truncates the terminal edge of the pyramid P—1 (*g*); and those faces which appear in the combination, will therefore be designated by $\frac{\breve{P}\bar{r}-1}{2}$. The inclination of *i* to *a* is 147° 52′. For the development of the pyramid *k*, we have two data in the situation of the edges produced in intersecting the faces of other forms. From the parallelism of the edges between *P* and *k*, *k* and *h*, *h* and *f*, &c. it follows, that the terminal edge of *k*, corresponding to the long diagonal of P, has exactly the same inclination to the axis as the analogous edge of that form. From the parallelism between *k* and *g*, and *g* and *m*, it appears, that the other terminal edge of *k*, which corresponds to the short diagonal of P, has the same inclination to the axis as the analogous edge of P—1; and the result of both these observations is, that the ratio between the lines $a' : b' : c'$ in the pyramid *k*, expressed by functions of the analogous lines in the fundamental form, will be $a : b : 2c$; or $\frac{2a}{2} : b : 2c$, which corresponds to the crystallographic sign $\frac{(\breve{P}\bar{r}-1)^3}{2}$. The edge in which two faces *k* and *k* intersect each other, is = 111° 32′. Of the three horizontal prisms *l*, *m*, and *n*, contiguous to the short diagonal of P, only *m* can be determined from the situation of the edges of combination between *m* and *g*; it is $P\bar{r} - 1 = 84° 30′$. Immediate measurement was resorted to, for ascertaining the dimensions of the other prisms, which were found to be $l = P\bar{r} - 2 = 122° 20′$, and $n = \frac{3}{4}P\bar{r} = 62° 24′$. The inclination of *l* to *a* is = 151° 10′; of *m* to *a* = 132° 15′; of *n* to *a* = 121° 12′. The scalene four-sided pyramid, to which belong the faces *o*, *o*, is perfectly deter-

mined by the parallelism between *o*, *e* and *o*, and by that between *h*, *o* and *b*. From the first of these data, it appears, that the terminal edge of the pyramid *o*, contiguous to the long diagonal of the fundamental form, is inclined to the axis under the same quantity as the analogous edge of $P+1$, and that, consequently, $a' : b'$ will be in the ratio of $2a : b$. But, from the second datum, it is evident, that the terminal edge of the same pyramid, contiguous to the short diagonal of the fundamental form, will be situated like the analogous line of *h*, which is $\frac{(\breve{P}-2)^4}{2}$; and this makes the ratio of $a' : c' = a : 4c = 2a : 8c$. The ratio between the three lines $a' : b' : c' = \frac{8a}{4} : b : 8c$, in the derived pyramid, is expressed by the crystallographic sign $(\breve{P}-2)^8$. There are some slight indications of faces between *f* and *o*, and between *o* and *d*; but they are too imperfect to allow of any accurate determination. The rest of the faces which appear in the combination, are easily referred to those forms to which they belong, because they possess on the opposite side of the axis a situation perfectly analogous to that of the forms developed in the preceding observations. Thus, *g'* is analogous to *g*, and therefore $= -\frac{P-1}{2}$; *h'* analogous to $h = -\frac{(\breve{P}-2)^4}{2}$; *k'* analogous to $k = -\frac{(\breve{P}r-1)^3}{2}$; *o'* analogous to $o = -\frac{(\breve{P}-2)^8}{2}$. The designation of the whole form is,

$$\underset{a}{P-\infty}.\ \underset{i}{\frac{\breve{P}r-1}{2}}.\ \underset{f}{\frac{\breve{P}r}{2}}.\ \underset{e}{\frac{\breve{P}r+1}{2}}.\ \underset{g}{\frac{P-1}{2}}.\ \underset{h}{\frac{(\breve{P}-2)^4}{2}}.\ \underset{k}{\frac{(\breve{P}r-1)^3}{2}}.\ \underset{P}{\frac{P}{2}}.$$

$$\underset{o}{\frac{(\breve{P}-2)^8}{2}}.\ \underset{l}{\bar{P}r-2}.\ \underset{m}{\bar{P}r-1}.\ \underset{n}{\tfrac{3}{4}\bar{P}r}.\ \underset{e'}{-\frac{\breve{P}r+1}{2}}.\ \underset{g'}{-\frac{P-1}{2}}.\ \underset{h'}{-\frac{(\breve{P}-2)^4}{2}}.$$

$$\underset{k'}{-\frac{(\breve{P}r-1)^3}{2}}.\ \underset{P'}{-\frac{P}{2}}.\ \underset{o'}{-\frac{(\breve{P}-2)^8}{2}}.\ \underset{c}{P+\infty}.\ \underset{d}{(\breve{P}+\infty)^4}.\ \underset{b}{\breve{P}r+\infty}.$$

The perfectly hemiprismatic appearance of a crystal similar to the last of these figures, would be alone sufficient for excluding the forms of Axotomous Lead-baryte from the rhombohedral system, even though the measures of the angles should have been found to approach still nearer to 120° and 90°. But one and the same individual seldom presents more than one or two of its six sides to the observer, being in most cases joined to other individuals, according to the law of regular composition mentioned in the beginning of this paper.

The planes of composition pass through a line nearly perpendicular to two sides of the six-sided tabular crystals, like AB, Fig. 8. They are parallel to one face of $(\breve{P}+\infty)^3$, = 119° 40′, a prism which is likewise found in the crystals of this species. Upon this supposition, the angles are = 90° 20′, and 89° 40′. Of the individuals AA′BCDE, and AA″BC′D′E′, which meet in the plane of composition passing through A B, nothing will remain but the rhomb-like trapezium AA′BA″, the angles of which are A = 60° 20′; A′ and A″ each = 119° 50′, and B = 60°. If a third individual A′A″CGFC′ joins the regular composition of the preceding two, being applied to BMA′, the remainder of BAA′, Fig. 8. in the line A′M, there will also arise a face of composition between A′MA″ and BMA″, and the angles of the remaining triangular figure A′A″B will be exactly = 60°. In the compound crystals of Axotomous Lead-baryte, each of the edges A′M, A″M, and BM is = 179° 10′.

The regular composition of three individuals of the variety Fig. 5., if they terminate at the faces of composition, will produce a form like Fig. 10., but the compound takes the appearance of Fig. 11. whenever the substance of the individuals reaches to the opposite side of the compound crystalline group, and thus produces faces of crystallisation on the other side, similarly situated, and parallel to those considered above. If, in the compound

masses, the faces of $\mathrm{P}\breve{\mathrm{r}}+1$ or *e* are considerably increased, while those of $\frac{\mathrm{P}}{2}$ or *P* disappear, the whole will assume, very nearly, the form of an acute rhombohedron, whose apices and lateral solid angles are truncated. The incidence of *e* upon *e′* is $= 72° \; 39'$, almost the same as the angle given by Mr BROOKE for the terminal edge of his acute rhombohedron. Even in crystals most perfectly formed, it is very easy to overlook the small salient angle of 179° 10′ upon the faces RST, supposed perpendicular to the axis of the rhombohedron; but this composition is often so intricate, particularly in larger crystals, that it sometimes becomes difficult to point out the direction and extent of each separate individual, though the existence of the composition is indicated by small, salient and re-entering angles, and proves, with the highest degree of evidence, that the forms of Axotomous Lead-baryte are not Rhombohedral but Hemiprismatic.

Although the observation of the optical properties of minerals can never supersede the study of their regular forms, yet the preceding examination of the forms of Axotomous Lead-baryte, affords an ample proof that they may be highly useful in guiding us through the latter, particularly if these regular forms nearly coincide with certain limits. The crystallographic researches relative to this species are attended with considerable difficulties, since the angles approach in every instance within one degree to the limits of 120° and 90°, and the regular composition very often hinders the crystals from being observed on all sides, while the inclination of the optical axes of no polarisation upon each other is very considerable, and easily ascertained.

The inferences drawn from Dr BREWSTER's general law, respecting the existence of two polarising axes in crystallised sub-

stances, had excluded the forms of Axotomous Lead-baryte from the Rhombohedral System, previous to their correct determination, and even in contradiction to the opinions entertained by crystallographers. The preceding demonstration, that they are Hemiprismatic, reconciles the results of both sciences.

XVI. *Inquiry into the Structure and probable Functions of the Capsules forming the Canal of* PETIT, *and of the Marsupium Nigrum, or the peculiar Vascular Tissue traversing the Vitreous Humor in the Eyes of Birds, Reptiles, and Fishes.* By

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(Read March 15. 1824.*)*

THE following additional observations on the comparative structure of the eye-ball, are intended chiefly to illustrate the philosophical anatomy and physiology of the capsules forming the Canal of PETIT, and of the Marsupium Nigrum; yet as I have here taken notice of several other points in the comparative anatomy of the eye, the memoir may be considered as supplementary to those formerly read to this learned Society, and which it did me the honour to insert in its Transactions *. I have stated in the first of those essays, that I had been led to inquire into the structure of the Eye, partly as connected with researches into the comparative anatomy of all the organs of sense, but more particularly with a view to elucidate the nature and distribution of the nervous system. I did not presume to think that any remarkable peculiarities in the structure of this most interesting organ, had escaped preceding anatomists; but though I found this to be true, in so far as regards the structure of the eye in most of the mammalia, it yet appeared that the same organ in other vertebral animals had by no means been investigated, or at least described, with the same care.

* Vol. x. Part i. p. 43.

Since that period I have repeated, with as much exactness as possible, most of the dissections on which my former essay was founded, and by employing very delicate coloured injections, with which the bloodvessels of the eye were filled in several animals, I have thought it might not be uninteresting to state briefly the result of these investigations to the Society, avoiding as much as possible all tediously minute anatomical details.

I. *Of the Retina.*

I regret that it has not been in my power to extend my researches into the structure of this most important membrane of the eye in the human subject; the obstacles in this country to such dissections being considerable, and not to be overcome by any individual not a teacher of anatomy: but I have seen enough to convince me, that the first views adopted by me, relative to the two most important points for investigation, viz. the foramen centrale of the retina, and the mode in which the membrane terminates anteriorly, are correct. Many of the members of this Society are no doubt aware, that two distinct opinions have been held relative to the nature of the discovery of SŒMMERING; some anatomists viewing the transparent point in the axis of vision, (which he supposed peculiar to the human subject), as a distinct foramen, or absolute perforation of the retina; others, as the immortal CUVIER, considering it merely as a transparent point, and that there is no real deficiency of the retina, but that the nervous membrane at this point merely remains transparent after the death of the animal, whilst the surrounding portions of the retina become opaque. They argue, therefore, that the *foramen centrale*, or transparent point of SŒMMERING, does not exist till some time after the death of the animal.

I hold this opinion to be altogether incorrect *, as applied to the pulpy or true retina, but readily admit that the perforation does not extend to the whole of the membrane usually called Retina. The retina is composed, in most animals, of at least two membranes †, viz. an external or pulpy layer, and an internal, (generally vascular), described improperly under the name of *tunica vasculosa Retinæ*. Air blown in betwixt the retina and choroid in the situation of the *Membrane of Jacob*, cannot pass into the chamber of the eye containing the vitreous humour, because it is arrested in its passage through the *foramen centrale* by the internal tunic of the retina. When I discovered that the *foramen centrale* of the retina was not peculiar to man and the quadrumana, as all anatomists before me believed; but that, on the contrary, it was extremely developed in the cameleon, and in certain lizards ‡ somewhat allied to the cameleon, I judged it a favourable opportunity for re-examining the subject with great attention. The structure was viewed under a good microscope, aided by a strong light, and submitted to a number of gentlemen well qualified to judge of such matters. Now, on this subject, there was but one opinion, viz. that the pulpy part of the retina in the situation of the *foramen of* SŒMMERING is absent, but that the

* See the facts stated in my " Account of the Discovery of the Foramen Centrale of the Retina, in the eyes of certain Reptiles," published in the Memoirs of the Wernerian Society, vol. 5. p. 1.

† I conclude that even when the retina has no longer a tunica vasculosa, as we find to be the case in birds, there still exist two layers, vascularity not being the essential character of either. Dissections by HALLER seem to confirm this idea; his words are, " Lamina ergo hic in retina interior fibrosa est (in piscibus), et alia exterior pulposa." But he allows that it is exceedingly difficult (he might have added impossible) to demonstrate this structure in birds; it must therefore be simply *inferred by analogy*.

‡ The Lacerta superciliosa, scutata, &c.

inner membrane passes on uninterruptedly *. So far, then, it would appear, that the transparent point of Sœmmering is occasioned by a deficiency of the pulpy portion of the retina.

We thus get rid of a war of words, which probably would have arisen from the fact of the internal tunic being continuous, which was never questioned.

The opacity assumed by the retina after immersion in spirits, is chiefly owing to its pulpy layer; and as this is wanting at the foramen centrale, or directly in the axis of vision, we readily perceive why the appearance should become more distinct after the eye has been immersed in spirits.

In answer to those who argue, that, as the retina is perfectly transparent during life, so the foramen centrale can be said to exist only some time after death, when the nervous membrane, becoming everywhere opaque, excepting in the line of the axis of vision, permits our seeing the choroid at this particular spot; I would reply, That such might be the case were it only a transparent point; but it has been already shewn, that the pulpy portion of the membrane is absent. I have, moreover, examined the eye-ball in a great number of animals immediately after death, and never found the retina to be absolutely transparent (excepting when dried), but uniformly of a bluish colour, and very slightly, though in general visibly opaque. The bluish cast I attribute to the subjacent choroid. This remark I have made on the eyes of several of the domestic animals, as oxen, sheep, horses, dogs, &c; on many birds, and particularly on fishes; and, lately, on man himself. For this I am indebted to Dr Monro, who, with his accustomed liberality, permitted me to examine the eyes of a man who was executed in this

* As I am assured that vascularity is not the essential character of this membrane, I shall prefer calling it by the name of the *inner membrane of the retina*: the reasons for so doing will be given afterwards.

place. The eye was opened in about eight hours after the death of the criminal: the foramen of SŒMMERING was remarkably distinct, and of a deep yellow tinge; *there was no fold*, a fact which proves this appearance to be a *post mortem* one, and that SŒMMERING has on this point misled all anatomists since his discovery. The retina was semitransparent. There were several vessels on the surface of the retina, apparently veins filled with dark blood; none could be discovered traversing the vitreous humour, for the reason, no doubt, that the branch which is found to do so in the fœtus of the mammalia, disappears in very early life, and perhaps soon after the structure of the lens is completed.

The uses of this most mysterious part of the eye-ball, and why it should exist only in certain classes of animals, and these, too, differing widely from each other, are physiological problems which seem to me of extremely difficult solution.

Several pellucid vessels or fibres seem to connect the retina and vitreous humor around, and near to, the entrance of the optic nerve. They are spoken of by HALLER in the following passage: "Vitrea membrana, simplex, intra retinam ex seipsa oritur, nusquam observabili aliquo vinculo connexa, nisi Albinianam arteriolam velis, aut vasa vitreæ membranæ pellucida, quæ in ove et bove ad eamdem retinam parallela sublinit, non obscura veniunt."

The pulpy portion of the retina is not of uniform density, nor is the membrane equally affected by the action of spirits: these render the greater portion of it perfectly opaque, and of a slight yellowish colour. But in the eye of the cat a large triangular space, whose apex is at the entrance of the optic nerve, and base externally and anteriorly, remains semitransparent, and comparatively much thinner, than any other portion of the retina.

With regard to the anterior termination of the retina, I may remark, that I have not found any reason to alter the opinions

expressed in my former paper on the Comparative Anatomy of the Eye. I therein state, that the pulpy layer of the retina terminates by a well-defined margin near to the place where the internal ciliary processes (Zonula Ciliaris of ZINN) commence; but that the inner layer of the retina may be considered as advancing forwards towards the lens, and uniting with the other transparent tunics to form the internal ciliary processes *, and the internal parietes of the Canal of Petit. I still admit, that this opinion rests partly on analogy, but on analogy of the strongest nature. We see the inner tunic advance forwards to the point where the internal ciliary processes commence; from this point forwards to the capsule of the lens, the structure has changed considerably; the membrane has become much more vascular, and it is folded into numerous plaits: but the difference in structure is not greater than what takes place in the corresponding coloured membranes; viz. the choroid and true ciliary processes; and I hold the analogy as to form, structure, and relation to their respective membranes, as complete. The fact that the retina separates very readily (after a little maceration) from the portion of the vascular tunic, which we may suppose as contributing to form the internal ciliary processes, merely shews, that, at this part, the membrane is very delicate, and easily torn, but perhaps not more so than at any other point of the retina.

This question was much disputed in HALLER's time: he adopted the opinion, that the inner or vascular tunic of the retina extends as far as the capsule of the lens; he even shewed

* These internal ciliary processes were called Fibres by ZINN, Lymphatic Vessels by BERTRANDI, and Tendinous Fibres by CAMPER. They are composed chiefly of arteries and veins. TENON well understood their anatomy, and describes their mode of union with the true ciliary processes, *and the important fact of their receiving bloodvessels from the latter organs.*

the membrane to ZINN; but that distinguished anatomist argued, that this was the *corona ciliaris, which he considered as distinct from the membrane of the retina.* M. DE BLAINVILLE adopts the opinion of HALLER. But whether or not we suppose the inner membrane of the retina to be continued as far as the crystalline, it is always to be remembered, that after it has quitted the pulpy layer of the retina, it assumes a new form, becoming exceedingly vascular, and has the same title to be considered as a structure distinct from the tunica vasculosa retinæ, as the true ciliary processes are from the choroid. I view both as merely appendages, and continuations of the respective membranes to which they belong. The intimate dependence of the internal ciliary processes on the vascular portion of the retina in the mammalia, may be judged of by the fact stated, that these processes cease to be vascular in birds, wherever the former no longer has distinct bloodvessels. Finally, I do not think that this continuation of the inner membrane of the retina contributes so essentially towards the formation of the Canal of Petit as does the continuation of the hyaloid membrane.

By the employment of delicate vermilion injections, I find that the ciliary processes of the vitreous humor (the Corona ciliaris of ZINN) abound with bloodvessels. I beg leave to present to the Society numerous preparations illustrative of so interesting a point in the anatomy of the eye *. The union be-

* The preparations illustrative of this and numerous other important points in the structure of the eye, amount to forty-seven. A far greater number was destroyed, in order to perfect the researches connected with this and the preceding papers. Of those preserved, thirty-six have been deposited in the Anatomical Museum of the Royal College of Surgeons of Edinburgh ; and the remainder, a distinguished oculist of this place did me the honour of placing in his own collection. These preparations are intended to illustrate every important fact in the Comparative Anatomy and Physiology of the Eye.

tween the true ciliary processes, and the ciliary processes of the vitreous humor, is chiefly vascular. Branches of the central artery and veins of the retina likewise anastomose with those received from the external ciliary processes: but such anastomosing vessels are comparatively few.

These dissections have explained to me a number of interesting facts in the philosophical anatomy of the eye, which previously I could not understand, as they seemed totally unconnected one with another.

The Canal of Petit, as has been stated, is formed by two layers or membranes quite distinct from each other. It is not of much importance whether we view these as merely a continuation of the hyaloid and of the inner membrane of the retina, (as I consider them really to be), or as being of a totally different nature from these membranes.

I have said in my former paper on the Comparative Anatomy of the Eye (p. 19.), that, "from the internal surface of the transparent ciliary body just described (the Zonule of Zinn) is detached a membrane, which being inserted into the capsule of the lens, somewhat more posteriorly or central, thus contributes to complete the triangular-shaped Canal of Petit." But the preparations I have now the honour to shew to the Society, demonstrate that *the membranes forming the internal and external parietes of the Canal of Petit, unite anteriorly with the capsule of the lens, and with each other, occasionally by a very acute angle; and that it is from this point that a membrane seems to be transmitted over the whole posterior surface of the capsule of the lens; but this membrane has nothing to do with the canal itself* *.

* We shall return to this fact more particularly in a future part of the memoir. The descriptions usually given of the structure and formation of the Canal of Petit, by anatomical writers and lecturers, are frequently quite unintelligible.

Both layers are vascular; the external one remarkably so, receiving innumerable branches, perhaps chiefly veins, from the coloured ciliary processes which are immediately superincumbent to them, and they anastomose, as has been already stated, with those branches, whether arteries or veins, which we find distributed to the inner membrane of the retina in the eyes of most mammiferous animals. But what purpose can this excessive vascularity in these internal ciliary processes serve? I was unable to answer this question satisfactorily to myself, until I ascertained that, in the eyes of birds, the most delicate injections cannot demonstrate any vessels either on the retina or on the internal ciliary processes, and that the canal of Petit can hardly be said to exist in these animals; that, in short, the whole structure is in them rudimentary; the vascular or active, and really essential part, being transferred to another organ.

I need scarcely recal to the remembrance of this learned Society, that it is principally by means of arteries that the various parts of the body are nourished, and that it is through the medium of veins and lymphatics that the superfluous parts are removed: hence, anatomists have at all times been anxious to demonstrate the presence of these organs in the various textures of the body, and numerous valuable physiological and pathological facts, have arisen out of such inquiries. There exist, however, even at present, great differences of opinion as to the source whence the humors of the eye are derived: the membrane which some have called the secreting membrane of the aqueous humor, seems to possess no such function *; whilst to

* I have shewn in my former paper, that, in the eyes of certain animals, the inner membrane of the cornea may be traced over the anterior surface of the iris, whilst in others we merely infer its presence by analogy. In fishes, in which the first arrangement is most distinct, the aqueous humor is very small in quantity. I readily confess, that I have not been able satisfactorily to make up my mind as to the source of

one very delicate branch of a small artery has been consigned the task of secreting and nourishing the lens, and perhaps even the vitreous humor; though, relative to this last, anatomists and

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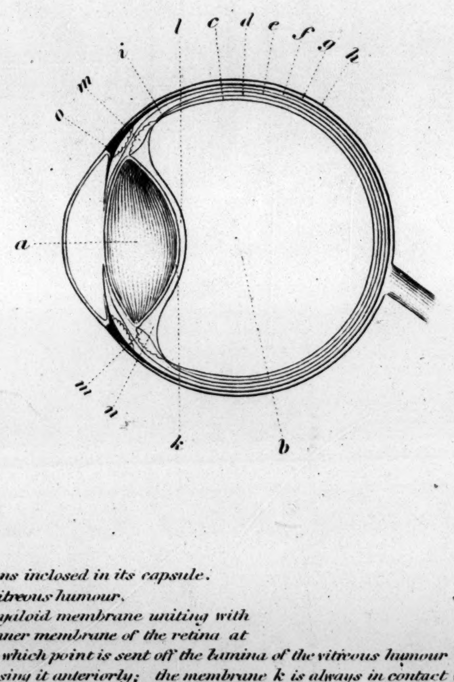
the aqueous humor; and the assertion, of its not being present in the anterior chamber of the aqueous humor in the fœtus, appears to me extremely doubtful.

In examining the eyes of the fœtus in the mammalia, with a view to the determination of the vascular structure of the pupillary membrane, several appearances have presented themselves to me, which I shall here briefly state, since they may be useful to those who may not have opportunities for making very minute vascular preparations of the eye. It has always appeared to me (and I have sacrificed a great number of very delicate preparations, in order to determine the anatomy of the pupillary membrane), that, by means of this membrane, the anterior and posterior chambers of the aqueous humor, form in the fœtus two distinct shut sacs, each enclosed in a proper capsule. With regard to the anterior of these, it is very evident from several preparations now lying before me, that it is formed by the inner membrane of the cornea, (or at least of a membrane covering the inner surface of the cornea); which is reflected over the whole anterior surface of the iris and pupillary foramen; but I cannot speak so decisively of the formation of the posterior sac, or that situated behind the iris, *i. e.* I am unable to say, whether it terminates at the equatorial margin of the lens, after investing, though loosely, the floating terminations of the ciliary processes, or whether it invests the anterior surface of the lens. It is very evidently connected with the membrane forming the canal of Petit. Betwixt the portion of membrane extending from the equatorial margin of the lens to the pupillary edge of the iris, (in the fœtus), and which being continued forward, constitutes one layer of the pupillary membrane, and the iris itself; there is a triangular space, very distinct in several preparations I have now before me. This space is occasioned by the membrane proceeding from the lens to the iris, in a straight line, like the string of a bow; whereas the iris is arched or concave. It is probable that the diagrammatic figure (Plate X.) may explain, much better than any words could do, the distribution of these membranes in the fœtus or very young animal.

The great share the hyaloid membrane of the vitreous humor has in the formation of these membranes, as well by its intimate connection with the capsule of the lens, as by its most evident and distinct connection with the posterior layer of the pupillary membrane, is very remarkable, when contrasted with the extreme delicacy of the vascular membrane of the retina, which scarcely contributes any thing towards the formation of this admirable and very singular structure. Viewing the interior of

 Eng.d for the Royal Soc. Tran. Vol. X p. 240



a......*The lens inclosed in its capsule.*
b......*The vitreous humour.*
c......*The hyaloid membrane uniting with*
d......*the inner membrane of the retina at*
i......*from which point is sent off the lamina of the vitreous humour*
k......*inclosing it anteriorly; the membrane k is always in contact with*
l......*which I suppose to be a very delicate membrane inclosing the capsule of the lens but this cannot very readily be demonstrated.*
m m..*The canal of Petit.*
n......*A cavity that can be formed by blowing air into it after rupturing the membranes forming the canal of Petit; it does not communicate with any other*
e......*The pulpy portion of the retina terminating in all animals where the internal ciliary processes commence.*
f......*The membrane of Jacob.*
g......*The choroid.*
h......*The sclerotic.*
o......*The annatus albus.*

W. H. Lizars sc.

physiologists, with the exception of HALLER, have preserved the profoundest silence.

When we consider the vast comparative size of the vitreous humor, we may feel assured, that one small branch of an ar-

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the eye-ball philosophically, we might say, that it is composed originally of a series, or suite, of colourless capsules, forming generally shut sacs, in which are deposited perfectly transparent substances, having various refractive powers. Posteriorly we have the hyaloid sac, and the contained vitreous body: *2dly*, The crystalline capsule and lens: *3dly*, The posterior chamber of the aqueous humour, formed in the fœtus by its own capsule: *lastly*, The anterior chamber, like the rest, a shut sac. The membranes forming these various capsules appear in the fœtus to be continuous.

In the fœtus, or very young animal, the quantity of fluid in either of the aqueous chambers of the eye, must be very trifling, if it actually exist; for the pupillary membrane is in contact with the anterior surface of the capsule of the lens. On the other hand, the crystalline humor in the fœtus is very large, and has the posterior segment of its capsule entirely covered with bloodvessels. The branch of the central artery of the retina which passes through the vitreous humor in a young animal, apparently quite disproportionate to the other branches of the arteria centralis, is for the purpose, undoubtedly, of furnishing the lens with the means of rapid growth. In the adult animal it is altogether obliterated, seemingly because no longer required, the lens having, at an early period of life, acquired its full growth, and being, like the enamel of the teeth, subject neither to decay nor renovation.

The vessels of the pupillary membrane come to it chiefly from those of the iris: it may receive a few from the terminating branches of the artery distributed to the capsule of the lens, but assuredly these must be very few. They were demonstrated by Dr W. HUNTER, who also describes, very accurately, the distribution of the posterior layer of the pupillary membrane: "Where the membrana pupillæ exists, there is a fine vascular membrane all around, which passes in the posterior aqueous chamber, from near the edge of the lens to the edge of the pupilla *." Now, it is extremely easy to demonstrate, that the membrana pupillaris consists of two layers; the posterior of which is a continuation of the one just described by Dr W. HUNTER. From the peculiar form of this membrane, (I mean the portion proceeding from the margin of the lens and hyaloid membranes to the pupillary margin of the iris), I should imagine it to be immediately ruptured and destroyed by the contraction of the iris, on the first admission of light through the pupil.

* Medical Commentaries, p. 63.

tery, (which artery is obliterated very early in life), is totally inadequate for its growth and support, even supposing that this vessel should entirely belong to it. But anatomists, anxious to ascertain the secreting vessels of the lens, and of its capsule, have pushed their researches relative to the branch of the central artery of the retina, which passes through the vitreous humor in the young of mammiferous animals, so far as nearly to have proved, that this vessel is ramified chiefly on the capsule of the crystalline. It is nevertheless true, that a few excessively delicate branches from the artery, seem to belong exclusively to the vitreous humor; but these are totally inadequate to the secretion of so large a mass, proceeding even on the supposition (in all probability incorrect), that the vitreous humor once secreted, becomes, as it were, a dead body, placed without the circle of the circulation, and no longer subject to decay or renovation.

There is still another source whence the fluid composing the vitreous humor might be derived. The branches of the central artery of the retina, which are distributed in a beautiful network over the inner tunic of the retina, proceed as far as the zonule of ZINN, and are then lost upon the external parietes of the canal of PETIT, and more particularly on those reduplications of this membranous body which I have called the Internal Ciliary Processes. Still these anastomosing vessels are few, compared with the almost innumerable branches the same processes receive from the true or coloured ciliary processes, of which fact I have already spoken.

Although these researches have as yet been, in a great measure, confined to the eyes of quadrupeds, and animals nearly approaching them in structure, I yet feel assured that the inferences are strictly applicable to the human eye. WALTER, as early as 1778, partly describes them in the following words: "Interna superficies membranæ choroideæ has conducit ad venas capsulæ

lentis et corporis vitrei quæ elegantissimum spectaculum quod nunquam satis verbis efferri potest, oculis exhibent."

Again, speaking of the veins of the choroid, he says, " Paulatim magis parallelæ fiunt, minutissimæ hæ venulæ si accesserint ad posteriorem terminum processuum ciliarium, ubi retina crassiori sua parte, ad limbum posteriorem corporis ciliaris adhærescit, et ubi retina subtilissimam lamellam emittit, quæ ut subtilissimum velamentum annulo mucoso, subjectam ad ambitum capsulæ lentis advenit, et ubi ea in convexitatem anteriorem transire incipit, huic adhæret." And a few pages further on he says, " Ex hoc reti admirabili ciliari, ubi illud apicem processuum ciliarium operit, exeunt minutissimi surculi qui subtilissimam productionem retinæ antea descriptam perforant, et paululum reflectendo, adeunt membranam corporis vitrei annulo mucoso tinctam, plurimi horum surculorum se immergunt in capsulam lentis, ubi ea, ope laminæ anterioris zonulæ ciliaris Zinnii, firmiter cum hyaloidea cohæret, qui profundius delati in posteriore convexitate capsulæ lentis terminantur."

I do not hesitate in ascribing to these processes, or at least to the vessels distributed to them, the office of secreting the vitreous humor,—of restoring it when partially lost, a process we know to be very limited,—and of absorbing, by means of proper vessels, the superfluous parts *. In birds, we find that the inner membrane of the retina is no longer vascular, and, indeed, is with difficulty demonstrated. I have not been able to

* It seems unnecessary to remark, that the secretion must ultimately be effected by means of vessels carrying colourless fluids, proceeding from those of a larger caliber, and distributed on the parietes of the canal of Petit. Vessels carrying colourless fluids no doubt penetrate into every part of the vitreous humor. Physiologists are as yet but little acquainted with the nature of the vessels carrying colourless fluids only.

satisfy myself of its existence. The most delicate arterial injections fail in shewing a single branch of a vessel carrying coloured fluids in the zonule of Zinn, or capsule of the lens of birds; but the *marsupium* or *pecten*, passing through the centre of the vitreous humor towards the lens, receives from the central artery a most abundant supply of bloodvessels: a few of them, though they must be very delicate, are seen to pass to the capsule of the crystalline *. Hence, it would seem, that the different arrangement of the vessels which occurs in the mammalia, and in birds, occasions a difference in structure; but that the analogies are closely observed. In man, and in some other animals, the vessels intended to nourish the vitreous humor are supplied to it chiefly by those sent from the ciliary processes, from the branches of the central artery of the retina which are distributed to its inner tunic; and, lastly, though I believe only in the young animal, from that branch of the central artery of the retina which passes through the centre of the vitreous humor. In birds, all these vessels are collected into one large group, and, as it were, projected through the mass of the vitreous humor; the vascularity of the inner membrane of the retina has disappeared, and the zonule of Zinn, so conspicuous for its vascularity and for the complexity of its structure in the mammalia, is reduced to a mere rudiment, destitute of bloodvessels. We are forced, then, to consider the marsupium as analogous to these organs; as being in fact their substitute; its principal, if not its sole function, being to support, nourish and absorb the

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* I am not sure if I rightly understand a passage in M. DE BLAINVILLE's work entitled, " Principes de l'Anatomie Comparée." He there states, that he has seen a bloodvessel entering the marsupium in the eye of the casuary; but several of the preparations exhibited to the Society, shew that sixteen or seventeen distinct vascular trunks may be counted in the marsupium of the common domestic fowl.

vitreous humor. Hence we see the reason why the marsupium does not extend in general as far as the capsule of the lens, an extent which its functions do not in the least necessitate. The same structure exists in certain fishes, and apparently for the same reason; but, on account of the extreme difficulty of injecting the bloodvessels in these animals, I am unable to speak from so great a number of observations as I could have wished *. The colour of the marsupium does not seem to be of so great moment as one would at first suppose. It is very generally dark-coloured throughout in birds, whilst in fishes it is most generally coloured only in a small portion, and is quite transparent near its base ‡.

I suppose the internal ciliary processes to be comparatively less vascular in man than in many other animals; still we know that the canal of Petit is very distinct, and no doubt has its parietes well supplied with bloodvessels. The appearances presented by these parts impressed on my mind the great superiori-

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* It would seem that in some fishes the vitreous humor is supplied by vessels in both ways †, *i. e.* a branch of the central artery of the retina passes directly into the centre of the vitreous humor, whilst the remaining branches pass into the same, but more anteriorly through the medium of the marsupium. They anastomose about the centre of the vitreous humor. We owe this fact to HALLER. It seems to me quite conclusive as to the real functions of the marsupium in fishes; yet this great anatomist calls the marsupium the *sustentaculum* of the lens, and says that in birds it carries the blood to the crystalline humor.

‡ The coloration of the iris, in itself a curious subject, is rendered more so by several facts, which do not very readily admit of explanation; such as the iris being differently coloured in the two eyes of the same individual, an appearance I have seen not unfrequently. QUINTUS CURTIUS relates, that the eyes of ALEXANDER the Great presented this appearance. TENON gives a very good engraving of an iris on which there may be seen the letter T very distinctly.

† Element. Physiol. t. v. p. 391.

ty, which simply extracting the cataract through an opening in the cornea, must have over couching, or, indeed, over any other operation for the destruction of the cataract, where the needle or knife comes in contact with the canal of Petit and the internal ciliary processes. I recommend this consideration strongly to the attention of the practical surgeon; for if my memory fails not, the surgeon, in performing the operation of couching, endeavours to pass his couching-needle just anterior to the termination of the retina. Now, this is precisely the spot which anatomy teaches us to avoid, because it is here that the texture is most complex, the vascularity greatest, and on the integrity of which the vitreous body depends for its support.

In my former paper on this subject, I ventured to suggest that the *marsupium* in the eyes of birds, is actually the *tunica vasculosa* observed in the retina of man and other animals, or at least the vascular part of that tunic; that in place of being expanded over the inner surface of the retina, the vessels were collected into a group or bundle, and in this manner pierced the vitreous humor, on their way towards its anterior part. I was ignorant, at that time, of the extreme vascularity of the internal ciliary processes. *This fact places the subject in a new light, and proves that the marsupium in birds and fishes is the substitute for the internal ciliary processes* *.

The preparations which I have the honour to submit to the consideration of the Society, prove the mode of formation of the Canal of Petit to be different from what has been generally described. I have found, that near the point where the

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* I do not mean that there are no branches proceeding to the retina in birds, but merely that they are so small as entirely to escape notice, and no longer constitute a network on the inner surface of the retina, as in the mammalia.

internal ciliary processes commence, and where the pulpy portion of the retina ceases, there is detached a delicate colourless membrane over the whole anterior concave surface of the vitreous humor, and which, as is very correctly stated in anatomical works, is merely contiguous to the posterior surface of the capsule of the lens, but is not in any way connected with it. This contiguity I perceived to be constant, by reason, probably, of the elasticity of the membranes I am next to describe, viz. those immediately forming the canal of Petit.

After the layer of the hyaloid membrane which invests the anterior aspect of the vitreous humor has been detached, the remaining structure passes on towards the equatorial margin of the lens; but, previous to being fixed into its capsule, it alters very much in appearance, and assumes a structure I have already often described. It divides into two distinct layers; the external or outer, constituting the internal ciliary processes, and an inner membrane, also vascular, separating the canal of Petit from a cavity, which may be formed artificially, by blowing in air betwixt the posterior surface of the capsule of the lens, and the hyaloid membrane on which this rests *. Just as the membranes forming the canal of Petit are about to reunite, they adhere firmly to the capsule of the lens, and seem to transmit another very delicate membrane, closely investing the posterior surface of that capsule. This is the whole mechanism of the canal †.

* Marked *h* on the accompanying figure.

† I should think Mr Jacobson in error, with regard to the openings he supposed he discovered in the canal of Petit. The membrane passing betwixt the internal ciliary processes, or its outer *paries*, is excessively delicate, and may possibly have been ruptured. On this subject Haller expresses himself very positively, and with much accuracy and brevity: " Flatu enim immisso adparet circulum Petit solum inflari, neque aërem aut in lentem subire, aut in vitreum corpus, eum ergo circulum

II. *Of the Membrane of Jacob.*

We owe to the excellent anatomist whose name this membrane bears, the first correct description of a most interesting texture of the eye-ball, which had been talked of before his day

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versus lentem capsula claudi. Deinde sola lentis capsula facile inflatur, neque aër aut in circulum, aut in vitreum transit. Denique etiam vitreum corpus aërem recipit, qui neque in lentem transit, neque in Petit annulum."

Haller's description of the canal of Petit is obviously incorrect; and Mr Cloquet, by copying this description, but superadding to it the well ascertained fact, that the layer of the hyaloid membrane covering the anterior aspect of the vitreous humor, that, viz. upon which the lens with its capsule reposes, does not adhere to the capsule, has become thereby quite unintelligible. I here quote both passages.

"Lamina posterior vitreæ membranæ discedit ad originem processuum ciliorium, et ad lentem introrsum etiam, sed paulo posterius advenit, recta protensa, et eam porro postquam attigit, tenaciter satis conjuncta, posterius includit.

"Inter has duas teneras laminas flatus potest immitti, qui circularem canalem, frenulis subinde adstrictum, efficit," &c. But I have shewn, that if a delicate membrane be actually stretched over the posterior surface of the capsule of the lens, neither this lamina, nor that placed immediately behind it, inclosing the vitreous humor, have any thing to do with the formation of the canal of Petit.

The passage alluded to in Mr Cloquet's work is as follows: "Au niveau des procés ciliaires, vers le contour du crystallin, cette membrane (hyaloide) se divise en deux lames; l'une passe devant la capsule de ce corps, et l'autre tapisse la concavité qui le reçoit en arriere. Il result de leur ecartement un espace de la forme d'un prisme circulaire à trois pans, complete par le circonference du crystallin. C'est cet espace vide qu'on appele Canal Godronne ou Goudronna, ou Canal de Petit."

Haller says that the canal of Petit is present in all quadrupeds; an observation which agrees with the almost innumerable dissections I have made of that organ. He thinks it totally wanting in birds: I have proved that the only important part entering into its composition, viz. its vascular part, is wanting in these animals, for this very obvious reason, that its place is supplied by the marsupium; but it seems to me that there still exists, as it were, a rudiment of the part, though by no means distinct.

as a structure of little moment, and had, indeed, been seen only in detached portions, and had, moreover, been entirely misunderstood *. The value of the discovery may be best understood by reflecting, that it immediately encloses the retina or sensitive membrane of the eye, and hence becomes of the greatest interest to the physiologist. In my former paper, I was inclined to view it as being perhaps the source of a portion at least of the *pigmentum nigrum*, in which opinion I was strengthened by viewing the membrane as extending in many animals as far as the edge of the pupil, (a view which I still adopt relative to it); but this opinion (in itself merely speculative, and connected chiefly with the development of the membrane in the eyes of fishes) a more deliberate and careful examination of the organ compels me to abandon. It is not improbable that the gentleman to whom we owe the discovery, and our most correct views of the subject, will himself resume the inquiry: in the mean time, I shall take the liberty of stating a few observations I have made relative to it, which seem hitherto to have escaped notice.

The membrane of Jacob is generally of a brownish colour, and sufficiently opaque to arrest the rays of light, supposing them to have passed through the semitransparent retina; hence we perceive that a part of the functions heretofore assigned by physiologists to the choroid must belong to the *membrane of Ja-*

I am still inclined to think, that a very delicate membrane is detached from the anterior termination of the canal of Petit, at the point where its parietes reunite, and incloses the whole of the posterior surface of the capsule of the lens, closely adhering to this capsule: it is very obvious that this membrane must be quite transparent, as it is in the immediate line of the pupil. It would be extremely interesting to know, whether it is this membrane, or the capsule itself of the lens, which, in certain diseased states of the organ, becomes vascular, thickened, and opaque.

* HALLER saw portions of this membrane, which he describes as a sort of inorganic mucus. His words are: " Ut magnas maculas nigras sæpe retinæ tunicæ ad

cob. In those animals in whose eyes there exists a tapetum, the membrane of Jacob is not absent, as I supposed *, but has the same degree of transparency as the retina. Lastly, In most animals it is of a deeper colour than in man. If to these we add the fact, *that it does not possess any bloodvessels*, and that the colouring matter is absent in albino animals †, most comparative anatomists will agree with me in thinking, that in the membrane of Jacob, we may perceive a structure, *the product of organisation, itself inorganic, and quite analogous to the coloured portion of the rete mucosum of the skin.*

III. *Of the Annulus albus.*

IN confirmation of a former conjecture as to the importance of this part of the eye-ball, I find that it is sufficiently vascular (perhaps nearly as much so as the iris ‡), in every portion ex-

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hærentes viderim, in homine, ave, quadrupede. *Eæ maculæ in pisce in membranæ speciem confluunt totamque retinam tegunt.*" It is very evident, however, from several passages in the *Elementa*, that, with many others, he confounded, in most instances, the membrane of Jacob with the pigmentum nigrum.

* Such was also the opinion of HALLER: " In animalibus quadrupedibus ab ea parte abest, in qua tapetum illud lucidum conspicitur."

† " In cuniculo albo pupillis rubris, niger iste mucus desideretur, eaque ipsa est ratio, quare per corneam membranam vasa retinæ et choroideæ percipias."

‡ The hypothesis that the iris is composed entirely of bloodvessels, chiefly arteries, is founded on the supposed correctness of some preparations said to be in the possession of PROCHASKA: those I have made, with very delicate injections, negatively disprove the hypothesis.

The mode usually adopted to demonstrate the circular and radiating fibres of the iris merits particular notice. If we hold the iris of man betwixt us and the light, there may be seen, near to its pupillary margin, a portion of its texture of a definite breadth, much more opaque than any other portion of the moveable iris.

cepting its base or point of insertion into the sclerotic: this seems (as we would expect) to be of a cartilaginous nature. I have found the annulus albus to be very fully developed in the dolphin, and repeated dissections have left no doubt in my mind of the extreme inaccuracy there is in viewing this body as a nervous ganglion *.

Now, this opaque circular stripe, which may be seen in the iris of man, of the quadrumana, and many other mammiferous animals, (very remarkably in the otter), and in almost all birds, is pompously called the Sphincter Muscle of the iris. But to demonstrate the radiating fibres, a little artifice is resorted to, violating at the same time, all the rules of philosophic anatomy. The iris of the ox, or of some other large quadruped, is selected, in which the parallel membranous striæ, so abundant on the choroidean portion of the iris, are remarkably striking, and these are called *the Radiating Muscles of the Iris.* These striæ are not distinct in man, and in several other animals, in whom the iris is extremely irritable; its choroidean portion becoming proportionally delicate †.

* Walter says, that, in the human eye, the retina does not receive any arteries from the central artery of the retina: "Retinam nullas accipere arterias a centrale arteria." He proves, that, by filling the ophthalmic artery with coloured injection, nearly the whole veins of the eye-ball are also filled; and it was by comparing such vascular preparations with those in which the veins only were injected, that he finally adopted the above opinion. Succeeding anatomists do not seem to me to agree with him relative to the distribution of the central artery of the retina.

I have made relative to these matters very considerable researches, but not on the human subject. I find that when the eye of a quadruped, as the ox, sheep, deer, &c. is removed from the head, and injected by the branch of the ophthalmic artery, proceeding to the eye-ball, the injection very readily fills, not only the arterial system, but also the whole of the vasa vorticosa, the vessels of the capsule of Petit, and a great portion of those supplying the iris; but to succeed perfectly, the injection must be thrown in with considerable force, and some of the vessels are generally ruptured. I have not succeeded so well as I could have wished in those preparations intended to exhibit the veins only. Hence I am not able to speak positively as to the relative development of the veins, compared with the arteries, in the capsule of Petit; it is not improbable, however, that they observe the same proportion as in the other parts of

† See Memoir read before the Royal Society of Edinburgh, entitled, "*On the Comparative Anatomy of the Eye.*" Page 43. *et seq.* of this volume.

the eye. I have not remarked in any adult quadrupeds, whose eyes were prepared by me for dissection, any vessel, or branch of a vessel, passing through the centre of the vitreous humor; the whole of the *arteria centralis retinæ* being apparently distributed, in the first instance, to the inner membrane of the retina. In birds, as I have observed, it is quite the reverse.

It is in the eye of the cat that most of the facts may be best observed; for, in the eyes of oxen, sheep, &c. the vessels are very apt to give way, and the injected fluid becomes effused into the canal of Petit and betwixt the membranes. The principal vascular union betwixt the two sorts of ciliary processes is close to the edge of the lens.

XVII. *On an Anomalous Case of Vision with regard to Colours.* By George Harvey, Esq. F. R. S. E.

(Read January 19. 1824.)

As the following anomalous case relating to the vision of colours, appears to possess some remarkable peculiarities, I have considered it of sufficient importance to be submitted to the Royal Society of Edinburgh.

J. B., aged 60 years, served in early life an apprenticeship to a farmer; but, disliking agricultural pursuits, became a tailor, and afterwards entered into the Navy, and served in several general actions. After quitting the sea-service, he resumed his trade, and in the employment of which he now continues. From his childhood, it appears, he was unable to point out colours by their proper names; or, excepting in a few cases, to distinguish one colour from another. From the nature of his avocation, this circumstance must have often been to him the source of much inconvenience; and during his whole life, he has found the utmost embarrassment from it. He has remarked, that his inability of distinguishing colours, has cut him off from the enjoyment of many innocent and harmless pleasures. If a painting were placed before him, abounding with the most beautiful varieties of colour, it would only present a dull and cloudy appearance; and hence he has never made a practice of stopping at print-shops, or of visiting any scenic representations. In early life, he once visited a panoramic exhibition, and he remarked, that his mortification was extreme, when he found every one around him delighted with the splendour of the scenes;

whereas, to him, to adopt his own words, the whole presented nothing but "a smoky appearance." The face of Nature also, which, to the perfectly organised eye, presents so many exquisite varieties of colour, and so many beautiful diversities of light and shade, has always appeared to him under a dark and murky aspect. While others have contemplated, with high gratification, the splendour of the setting sun, or the glory of the rainbow, he has seen but little to admire; and, when led by the chances of a seaman's life, into the Mediterranean, where a bright sun, and a pure and cloudless sky, lend to the glowing tints and the vivid colouring of Nature, charms unknown to the climates of the North, the contrast produced no peculiar effect on him: nor has this arisen from a morbid constitution of mind; for, on the contrary, he is remarkably happy and cheerful; and, from all the information I have been able to obtain respecting him, has always been distinguished for his steadiness, cheerfulness, and good conduct.

From several opportunities that I have had of examining into the peculiarities of his case, I have drawn up the following brief observations.

Of Whites, he appears to have a very good idea, and so also of Greys;—he having selected five pieces of cloth of the latter colour, and arranged them according to their varieties of shade, with perfect ease. By candle-light, however, he mistook a thread of pink silk for grey, and, under the same circumstances, confounded flax-flower blue (No. 29. WERNER's Nomenclature of Colours by SYME) with the same colour.

On SYME's page of Blacks being presented to him, he thought the whole to be dark-green. At first he pointed out the specimen which had the darkest tone; but after a few minutes, he remarked, that they all appeared the same. When specimens of basalt and hornblende were placed before him, he could perceive

no difference, although the former had a greyish, and the latter a greenish hue. Between raven and fine velvet black he could perceive no difference. His master furnished me with the following fact. Being desired to repair an article of dress that required black silk, he employed crimson; and a similar mistake occurred on two other occasions.

Both indigo and Prussian Blue (24. and 25. of SYME) he regarded as black. China and azure blue (26, 27. *ibid.*) he considered to be blue, but thought them good matches for carmine-red (90.), when placed by its side. Ultramarine blue (28.), he thought to be the same as lake-red; and when a light lake-coloured wafer was laid on a piece of azure-blue cloth, he thought the resemblance very perfect. Flax-flower blue (29.), he could distinguish as blue. His master informed me, that he once confounded sky-blue with green when repairing some article of dress; and on another occasion, when a young gentleman's dark-blue coat was brought to him for immediate repair; the mother of the lad was surprised to find the elbow of the coat repaired with crimson *.

SYME's Purples he regarded in general as blues; the only one to which he could perfectly adapt his notion of that colour, being the imperial purple (39.) of a specimen of fluor-spar. To the pale blackish purple, the colour of the porcelain jasper, he would only give the name of dark colour. A blue lilac he called a lead colour; nor could he trace a shade of purple in the sweet-scented violet, or in a plum. From all the information I could obtain, he regarded purple as a slight modification of blue.

Greens were to him a source of much embarrassment. On a particular occasion, I requested him to bring me eight or ten

* In one of Dr NICHOLLS' cases, published in the Medico-Chirurgical Transactions, the following anomalous circumstance is recorded. "CHARLES was in the "Navy, and, several years ago, he purchased a blue uniform coat and waistcoat, "with *red* breeches to match the blue."

specimens of cloth, of different shades of that colour. This, I found from his master, was the occasion of much uneasiness to him; and he at last was compelled to ask one of his fellow-workmen to point out the green bundle to him, although they had been charged not to assist him in his difficulty. His master having discovered this circumstance, substituted some pieces of black and brown for some of the greens; and he, unaware of the change, furnished the following as varieties of green.

Four specimens of dark bottle-green.

One	reddish-black,	(21)	SYME.
One	raven-black,	(22)	
One	liver-brown,	(104)	
One	blackish-brown,	(108)	

When SYME's specimen of verdigris-green was placed before him, he declined giving any name to it, but remarked that it was certainly not green. The beautiful green of the emerald (52.), he called pale orange; and to grass-green he applied the same remark as to verdigris. Duck-green, which forms so interesting a feature in the neck of the mallard, he named brown or green, displaying much uncertainty; and the same ambiguity was manifested, when olive-green was shown him. On another occasion, being requested to point out two colours in the page, that resembled each other, he immediately fixed on the two last-mentioned, and again called them brown.

All his ideas of green appeared to be extremely confused. On being told that SYME's specimens were varieties of that colour, and requested to point out one that bore a resemblance to the green fields, he expressed his surprise at the remark, and contended that they bore no resemblance to it. The darker kinds of green he considered to be brown; those of a middle tone ambiguous; and the lighter kinds, as in the case of emerald-green, of a pale orange colour.

He experienced no difficulty with any of the brighter varieties of Yellow. From a number of pieces of different coloured cloth, he immediately selected a specimen of this colour; and a fragment of high-coloured sulphur he thought a beautiful example of the same. Gallstone-yellow he conceived to be orange. Wax-yellow (13.), which, in the Vegetable Kingdom, exists in the greenish parts of the nonpareil apple, he supposed might be a green. His ideas of yellow were, however, on the whole very correct.

His notions of Orange were very imperfect. The common marigold, he called Yellow; and a sample of fine orpiment, Orange; and likewise equally choice specimens of reddish and deep reddish orange he termed Brown.

Of the Reds, he considered carmine, lake, and crimson to be blues;—the latter, indeed, a dark-blue, agreeing with the instance of the coat before alluded to. When a great snapdragon was placed before him, he called the margin of its upper lip, which was purplish-red, a dark colour, and thought it a very good match for my black coat. The part also near the throat of the corolla, and which was of a light bluish-red, he called light sky-blue. The yellow palate he distinguished perfectly; but as that colour gradually lost itself in the purplish-red, he gave it the name of *black*. He remarked, that, when a boy, the ruddy cheeks and arms of the milk-maidens, always appeared to him of a bluish tone; and, on being shown a rosy child, he persisted in the same remark. The carnation, pink, and the cock's-comb presented also the same appearance. Scarlet-red he distinguished readily by its proper name. Veinous blood * he assimilated to brown or black; and the brown disk of the common

* Mr Dalton, in vol. v. p. 33. of the Manchester Transactions, remarks, that, in his own case, blood appears not unlike bottle-green; and, it is worthy of notice, that, in the present case, it was termed, "not a black, but nearly so."

marigold, and the iron-flint, although presenting so marked a difference to the perfectly formed eye, appeared to display no variety to him. To the latter, indeed, he gave the name of *olive*. The colours of the common garden rose and peach blossom, were both designated lead-colour. To him, therefore, some of the sweetest and most delicate colours of creation presented but little beauty.

In the case of Browns, there was much uncertainty; in the greater variety of cases assimilating them to green. Mineral pitch, although clearly a variety of brown, he considered to be black; and liver-brown he designated in the same manner. Chesnut-brown he could not distinguish. An article of dress, indeed, of the latter colour, he repaired with silk of an olive-green; and, on another occasion, he considered covered buttons of a bottle-green, as a perfect match for a dress of a blackish-brown. Two fragments of cloth, one a duck-green, and the other a liver-brown, were placed before him, and he was unable to point out the difference.

From his having regarded crimson-red both as dark-blue and black, it was anticipated that he would confound the two latter. This accordingly took place, when the indigo and Prussian blues of SYME's Nomenclature of Colours were shown him, calling them both black, notwithstanding they exhibited a marked distinction in that excellent work.

From the preceding observations it appears, that the only colours he was capable of distinguishing with certainty (by day), were White, Yellow, and Grey; and that in the proper perception of the following colours, there appeared to be varied degrees of uncertainty.

Colours proposed for Observation.	Perceptions of the proposed Colours.
BLACK.	Generally Green, in particular cases Crimson.
BLUE.	Darker kinds, Crimson and Black. Those of a middle tone Carmine and Lake. Those of a lighter kind able in general to distinguish.
PURPLE.	Appeared to be Blue, excepting in the case of a very bright Purple.
GREEN.	Confounded in general with Black and Brown. Greens of a darker kind appeared Brown. Those of a middle tone uncertain as to name. Greens of a lighter kind Dark-Yellow.
ORANGE.	Darker kinds Brown. Lighter kinds Yellow.
RED.	Carmine, Lake, and Crimson appeared Blue.
BROWN.	Green.

Since the above was written, the following circumstance occurred. He was sent to obtain some patterns of green baize, and having procured five varieties, placed them on his return in the shop-window. In the course of the day, his master called on him, to state the prices of the patterns he had obtained, at the same time placing before him two pieces of crimson cloth, and three of the green baize, and he, unconscious of the difference, fixed prices on each.

On being informed of the circumstance, I prepared two excellent specimens of the above colours, and placed them before him, first on a white ground, secondly on a yellow ground, and, lastly, on one of a green colour, and, in each case, he regarded the two colours, to use his own words, "as well matched." *

* A philosophical friend who was present, placed the colours under several different circumstances, but with the same uniformity of effect.

The day being favourable on which the last experiment was performed, a vivid and well-formed solar spectrum was thrown on the wall. He pronounced it to be composed of two colours, yellow and light blue; and which, in a former conversation, he described as the ordinary appearance of the rainbow. The vivid and brilliant red of the spectrum he could by no means distinguish. Its general appearance he regarded as in some degree beautiful; but the bursts of pleasure which escaped from my children, as they contemplated the brilliant colours in succession, appeared to excite in him the greatest surprise. I afterwards found, that he considered the prism as a thing moderately interesting, but as by no means meriting the praises which had been bestowed on it.

His eyes appear to be exceedingly well formed, and time has but slightly impaired their powers. According to the opinion of my friend MR TRACEY, surgeon, they appear to possess all the essentials necessary to good sight, namely, *perfect transparency of the several humours, a proper degree of convexity of the cornea and ball of the eye*, and to which may be added, *the perfectly healthy functions of its appendages;* the proofs of which are discovered in the just adaptation of the eye to distance. In his communication to me on the subject, MR TRACEY observes, " If I might adduce any one point (which, under common circumstances, I should not be disposed to notice) I should say the grey colour of the irides is much fainter than usual; and that the pupillæ are extremely small." The cause of the last-mentioned fact, is no doubt to be attributed to the constant exercise of the eyes by candle-light, it being known, that, persons similarly occupied, are of necessity obliged to bring the object very near, and thus, from long habit, produce artificially a permanent diminution in the magnitude of the pupillæ.

In the present limited state of our information on this very interesting and curious subject, and with so few cases that have

been hitherto presented to the philosopher, it may be premature to offer any very decided observations on the cause. The objects of a true and legitimate philosophy are perhaps best fulfilled, by diligently collecting facts, and, by cautiously deducing inferences from them, to form gradually the successive links of a chain, which shall ultimately lead to the true cause.

Of the hypotheses that have been proposed to account for the phenomena in question, that which refers it to an insensibility of the retina to certain impressions of light, appears at once the most simple and philosophical. Some eyes, it is well known, are capable of performing the general functions of vision, and are yet unable to perceive those minute impressions of light, which to other eyes are readily perceptible. A retina may be perfectly adapted to receive the due and proper effect of a beam of light, and yet, from some peculiarity in its organization, incapable of perceiving all its component parts. In the present case, the general objects of vision, as relate to form, distance, and magnitude, were perfectly fulfilled, and the sensations arising from white, yellow, grey, and the lighter varieties of blue, appeared in general to be correct; and it would therefore appear probable, that the retina was sufficiently sensible to receive correct impressions from pure white, up to colour of a certain intensity; and, beyond which, its power being enfeebled, it communicated only imperfect and confused ideas *.

In the cases published by Dr Nicholls, in the Medico-Chirurgical Transactions, the anomaly appeared in some measure hereditary; in one instance being derived through the father, and

* The point where this change appeared to take place, with respect to blue, was between ultramarine and flax-flower blue, corresponding to 28. and 29. of Syme.

in the other through the mother. In some families, this peculiarity of vision appears to be transmitted through a son, without affecting in the smallest degree a daughter; and in other cases precisely the reverse. In the present instance, however, the anomaly appears to have originated with the individual, no member of his family being similarly circumstanced,—he having always regarded himself as a singular and unfortunate exception to the whole.

PLYMOUTH, *July* 20. 1823.

XVIII. *Observations on the Germination of the Filices.* By the Reverend John Macvicar, Dundee. Communicated by the Reverend John Fleming, D. D. F. R. S. E. &c.

(Read June 6. 1824.)

Since the perfecting of a Natural Arrangement has become the grand object of scientific pursuit, it has been found necessary to enter more minutely than was required by an artificial system, into modifications of structure and function. Botanists have no longer rested satisfied with observing plants in their fully developed forms, but have also had recourse to several other periods in the existence of vegetable life, the observance of which gives much insight into the organisation of the individual. Of these there is none more important than germination, which is often the only means afforded for ascertaining the structure of the seed. This is the case with all the cryptogamic tribes, the seeds of which are generally so minute, that many of them must be brought together before they be distinctly visible. With such subjects dissection is hopeless, and a knowledge of their structure can only be derived, during their formation and evolution. The observations on this subject are as yet few, and those that have been made do not accord well with each other. Such at least is thought to be the case with the evolution of the Ferns. It appears, however, by a repetition of the experiments, and an uninterrupted series of observations on the evolution of the sporule from first to last, that the discrepance of observation arises, not from any inaccuracy on the part of any observer, but by each looking at periods of evolution different from the other; by do-

ing which, each saw forms apparently irreconcileable, and by a law of our nature, which requires us to believe the testimony of our own senses in preference to that of another, late observers have been led to attach doubts as to the accuracy of preceding observations, which nothing but a different result, obtained by the same process, could vindicate.

It has been the fate of LINDSAY, the first observer of importance, according to the opinion of late and far more enlightened naturalists, to see "several whimsical shapes *," which succeeding observers have not been able to recognise †. His observations ‡ were made at Jamaica some time previous to the year 1789, and the engravings were executed in England, where he had no opportunity of superintending, so as to correct errors. Notwithstanding, the figures are tolerably accurate, and certainly entitle the author to more merit than has of late been awarded to him. They commence with the sporule soon after germination, and he gives several representations of the plantule in successive stages, as seen by a high magnifier. These correspond to the first seven figures in the accompanying drawing; and, except that he has represented the radicles in all of them as proceeding from the sporule alone,—an error, it must be confessed, which of itself shews that he did not understand the peculiarity of the evolution,—they are not very unlike what the author has observed. He then permits some weeks to pass without observations, until the little plants begin to appear to the eye as small heart-shaped scales. His observations then correspond to the accompanying Figures 10, 11, 12, 13, 14, Plate XI.; they are represented of the natural size, and are tolerably just. The

* Sup. to Encyc. Brit. Art. Anat. Veg.

† Edin. Encyc. Art. Filices.

‡ Lin. Trans. vol. ii. p. 95.

greatest fault in his observations is, that he neglected to observe a most interesting process in the evolution, and by proceeding *per saltum* from the period at which a high magnifier was necessary to render his early forms visible, to the time when these appeared to the eye as small scales, he has represented forms nowise connected with each other, which has led late observers to doubt their accuracy. Of these, the most distinguished is the author of the article Filices in the Edinburgh Encyclopædia, published in 1815. The experiments which he there describes commence soon after what he terms the "seed-lobes" become visible to the naked eye. Of course they correspond to the second sets of Lindsay's observations, and, though much more perfect, are on a general view pretty similar. He has remarked the temporary radicles, the true root, and the elevation around the opening formed by the emission of the true frond, on all of which Lindsay is silent. Besides these, several others have noticed ferns in the act of germinating. Ehrhart seems to have observed some of the earlier forms of Lindsay. Sprengel saw a single species in a state of spontaneous germination, in its farthest advanced states, forms in which young ferns are to be seen in every shady moist crevice, or other place where ferns grow. Mirbel has also observed some of the later stages, differing in some unimportant particulars, in the form of what have been named the Seed-lobes.

Notwithstanding, however, that observers have been so numerous, it does not appear that the true mode of the evolution has as yet been understood. Lindsay makes no attempt to reconcile his early with his later forms; while the author of the article Filices, though he made experiments on various genera, could never observe the first forms of Lindsay. These circumstances are easily explained, however, if the appearances presented during the evolution of *Polypodium vulgare* be similar to

those of the other individuals of this natural family; and this we may safely conclude of all those at least having prostrate stems. The accompanying figures are representations of what the author has observed in that species. In the explanation, no dates are given, as the progress of germination depends entirely on the age of the sporule, the supply of moisture, temperature, &c. which are the measures of the progress of vegetation rather than of equal spaces of time.

After seeing the observations in the Edinburgh Encyclopædia, it occurred to me that the seed of the Filices must consist chiefly of two cotyledons folded together, which, by the absorption of moisture, and the germinating power, resolved themselves into the forms represented by the author of that article; an idea in which I was confirmed by observing the sporule of Polypodium vulgare, which appeared under a high magnifier curiously folded together like the surface of a brain. Whatever the surface of a sporule may be, however, the idea that it consists of two folded cotyledons is altogether erroneous. The opinion of Gærtner, that the testa of the seed is in them as well as in the Musci and Fuci, totally occupied by what he terms *vitellus*, seems much nearer the truth. The evolution of the Musci, indeed, has so many points in common with that of the Filices, as to indicate a similar structure of the sporule. Hedwig, and, after him, Mr Drummond of Cork *, when observing the young shoot from the seed of Funaria hygrometrica, saw that it had ruptured the integument, which, until more extensive experiments be made, may be considered as an epidermis. But whether this coat be, as in the higher vegetables, an integument of a different structure from the enclosed parts, and existing previously to the action of atmospherical causes, or whether it be a natural change

* Lin. Trans. Vol. xiii. Part 1.

produced by their action upon the uniform substance of the sporule, after the soft matter in the theca has resolved itself into little concretions, conducing powerfully to the preservation or the vital principle in the interior, and similar to a change observed to take place on the surface of Vorticella rotatoria, and other aquatic animalcula, when, by some accident, they have been exposed to the action of the air, and by which the principle of life has been wrapt up for several years, is not determined. The sporules of these two acotyledonous tribes seem to be small concretions of matter produced in the reproductive organs of most species, possessed of the vital principle of those species without the action of sexual parts, and of the power of giving rise to forms of germination peculiar to the family to which they belong; which intermediate forms, in their turn, and after the action of the sporule has ceased, have a similar power of evolving the mature forms of the species. The irregularity in size, form, and surface, observable even from the same theca, seems to countenance this idea of the simplicity of the sporule. In Polypodium vulgare they are comparatively large; they vary in surface, from nearly echinated to nearly smooth, and in form from globose to reniform. In Pteris crispa they are generally equilateral triangular pyramids, more or less perfectly formed; but they vary from having plane sides and sharp angles to globose.

Still, however, though this simplicity of structure were granted to the sporule of the Filices, it does not follow that this magnificent family must be ranked with the Acotyledones. After leaving the Dicotyledones, we do not observe the same uniformity in the structure of the embryo. Brown, our illustrious countryman, has shewn *, that certain Aroideæ produce seeds which have an appearance and economy much more nearly resembling the tuber of a root; for instead of being distinguishable into a

* Lin. Trans. vol. xii.

cotyledon, a plumule and radicle, and of germinating in a determinate manner, and from a single point; they are composed of a mass, whose internal structure is uniform, and on the surface of which frequently more than one germinating point are visible. And, there is reason to believe, that there are other families in this class, the structure of whose seeds is equally anomalous.

The author of the article alluded to was led to conclude, from his observations, that the seed-lobes of ferns possess not only an analogy with, but partake of the essential properties of cotyledons, as far as these have been accurately defined; and though he mentions some respects in which the germination of ferns is peculiar to this family, he gives particularly three in which it agrees with *other* dicotyledonous plants. The first is, that the seed-lobes constitute the body of the minute seed. Should the general mode of germinating throughout the family be uniform, of which, as that writer remarks, there can be little doubt, this cannot be the case; for in every instance where the sporule has been seen, and as long as it has been observed, it remained of nearly its original form and size, which could not have happened, had not what have been considered as seed-lobes been solely the result of germination. The other two are, that these seed-lobes nourish the other organs of the embryo, which they include within their substance; and that these organs germinate from a tuber situate in the centre of the lobes. They certainly do nourish the true bud, after they have disposed of the nutritious matter derived from the radicles for their own aggrandisement, until the law of their life does not permit them to increase in size, but not till then do they form a bud; and their action in doing so, is not by yielding any albuminous matter contained in their substance, as in true cotyledons, for they are thin cellular membranes, and their function seems to be to convert the fluids received from the numerous radicles on their under surface, into the forms of the species.

Were we to seek for analogies to these hepaticoidal forms of ferns, in more highly organised bodies, we do not know where to find them beyond the limits of this family. But there are several circumstances which might lead us to view them as representatives of the prostrate stem. They originate from the sporule, a minute tuber; they soon after emit radicles, which penetrate the soil, and contribute to their increase, and, when arrived at sufficient strength, they form a bud. And perhaps it may be found, by future observations, that the elongation of the hepaticoidal form, and the successive emission of radicles farther up, are confined to those species having prostrate stems.

It is not among the Dicotyledones that the Filices lay any claims to rank. Some most eminent botanists, however, have classed them among the Monocotyledones; a place assigned them chiefly from the structure of the stem, in which the occurrence of vascular fasciculi, and certain modes of growth, indicate many points common to them and to species decidedly monocotyledonous. But the detection of vessels in the stems of Ferns, and in other nearly related tribes, may have arisen from the circumstance, that they exist there in a state easily detected, while the minuteness of some other families very much limits the success of dissection. In this question habit gives little aid; for the gigantic arborescent species seem to approach as nearly to the magnificent Palms on the one hand, as the humble Trichomanes and Hymenophylla do to the Mosses on the other. But while the whole reproductive system is truly cryptogamic, and the object of the botanist to confine to one of three classes the almost infinite modifications of structure in the vegetating world, it seems preferable, at least, until the structure of several other cryptogamic tribes be investigated, to attach that importance to the fructification which has usually been given it, and to place them, where they have generally stood, among the Acotyledones;

although, if authority were sufficient to satisfy the doubts of all, their station should be among the Monocotyledones, having been placed there by the two greatest botanists of the age, Brown and De Candolle.

EXPLANATION OF PLATE XI.

Fig. 1. A sporule of Polypodium vulgare begun to germinate. The frondose portion is somewhat advanced before any radicles are emitted. Figs. 2, 3, 4, 5, different stages of progress. Fig. 6., at this stage the cellular structure becomes distinct, and radicles are emitted from the lower portion of the frond. Figs. 7, 8., the fronds now become emarginate, and curiously apiculate. The radicles are emitted from the margin and under surface. Those from the sporule and lowest part of the frond begin to shrink, along with that portion of the frond, as in Fig. 9., until, as in Fig. 10., the sporule is separated.

Fig. 11. The true frond beginning to shoot out from the centre of the hepaticæ-like form (copied from the Edinburgh Encyclopædia, Art. Filices).

Fig. 12. Plantule of *Aspidium Filix fœmina*, (copied from Sprengel's Introduction to the Study of Cryptogamic Plants).

Fig. 13. Plantule of *Polypodium lycopodioides* (copied from Lindsay's Observations, &c. Lin. Trans. vol. ii.).

Fig. 14. *Aspidium aculeatum* (Lightf.) in a state in which they are easily found growing spontaneously.

The first ten figures are magnified.

PLATE XI.

Engraved for the Royal Soc. Tran. Vol. X page 270

W. H. Lizars sculp^t

PLATE XII. *Eng.d for the Royal Society Tran. Vol. X. P. 2. P. 271*

Allanite

Fig. 1.

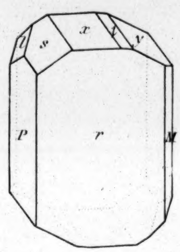


Fig. 2.

Fergusonite

Fig. 3.

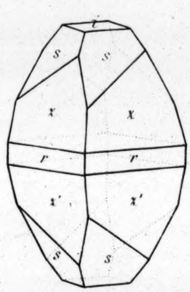


Fig. 4.

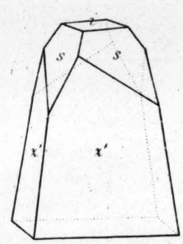


Fig. 5.

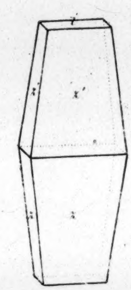


Fig. 6.

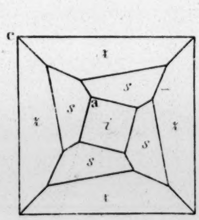
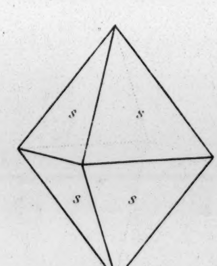


Fig. 7.



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W. Miller sc.

XIX. *Description of* FERGUSONITE, *a New Mineral Species.*

By W. HAIDINGER, ESQ. F. R. S. EDIN.

(Read January 17. 1825.*)*

FEW cabinets of minerals contain so many of the rare substances, which were discovered in Greenland, by Sir CHARLES GIESÈCKE′, and perhaps none in equally interesting varieties, as that of Mr ALLAN. The most prominent of them have been examined both in a mineralogical and chemical point of view, and some described as particular species. Mineralogy is already indebted for Allanite, Sodalite, Eudialyte, and Gieseckite, to the zeal of the indefatigable explorer of Greenland, and the subsequent labours of Dr THOMSON and Professor STROMEYER; and it is probable that the number of new species from this source will yet be increased upon farther examination.

Allanite had first been noticed by Mr ALLAN, who described it as crystallised Gadolinite *,—a variety of which it had been considered by Count BOURNON, resting, in part, upon some partial chemical experiments. Dr THOMSON† afterwards analysed it, found it to be a species of its own, and gave to it the name of Allanite, under which it has been since received in most of the treatises on mineralogy. The description of the crystals, given by this author, and the second figure which accompanies his paper, are perfectly correct, and refer to that variety of the mineral which he has analysed. It is well known at present, that the first figure represents a crystal of Zircon of a very dark colour. This

* *Transactions of the Royal Society of Edinburgh*, Vol. VI. p. 345.

† *Ibid.*, p. 371.

species occurs along with the Allanite, and sometimes imbedded in it, in small but very distinct crystals, generally of the form of a four-sided prism, terminated by a four-sided pyramid, in parallel position, and having occasionally its lateral edges replaced. Haüy, and most of the other mineralogists, in the description of Allanite, have quoted the measurement of the prism 117°, as indicated by Dr Thomson. Messrs Phillips * and Brooke † ascribe to it a rectangular four-sided prism, which is incompatible with the real form of the mineral. That substance, the form of which Mr Phillips has described, and which he calls Crystallised Allanite, does not belong to this species: it is a species of its own, but, like the Allanite, it belongs to the order Ore, of the system of Professor Mohs.

The more particular object of the present paper, is to describe this new species, for which, at the suggestion of Mr Allan, I propose the name of *Fergusonite*, in honour of a gentleman too well known to the mineralogical world at large, and to the members of this Society in particular, to require, in the present place, a more detailed acknowledgment. In order to produce a more distinct idea of the differences existing between the two species, I shall begin with giving a short description of the Allanite, and then add the description of Fergusonite, as derived from the specimens observed in the cabinet of Mr Allan.

I. Allanite.

Its form is tetarto-prismatic. Plate XII. Fig. 1. shews the elevation of a crystal. Fig. 2. the projection of the same, upon a plane perpendicular to the sides of the prism. It is the same crystal which has been mentioned by Dr Thomson, and presents

* *Elementary Introduction to Mineralogy*, p. 264.

† *Familiar Introduction to Crystallography*, p. 458.

a greater number of planes than any of those which have been described by Count Bournon. I have noted the faces, with the letters received for Axinite by Haüy, and in the Treatise on Mineralogy by Professor Mohs, for the sake of an easier comparison of the analogies among the crystalline forms of the two species. I obtained the following measurements of the angles:

r on $M = 129°$	y on $r = 109°$
r on $P = 116$	s on $x = 156\frac{3}{4}$
M on $P = 115$	x on $t = 164\frac{1}{2}$
s on $r = 135\frac{1}{2}$	x on $y = 151$
d on $r = 124\frac{1}{2}$	t on $y = 166\frac{1}{2}$

These angles are given merely as rude approximations to the true angles of the crystal, as, besides the use of the common goniometer, I was obliged to take impressions from them in sealing-wax, to make them at all fit to be measured with the assistance of the reflective goniometer. The surface is far from presenting a good polish, or high degree of lustre; the inclined faces s, x, t, and y, are more perfect in this respect than those which are parallel to the axis. The face M in particular is very rough and uneven. The inclination of r on P is more easily ascertained. The edge between P and M is also sometimes replaced by a rough face.

Faint traces of cleavage are observable parallel to P and r; but they are very indistinct and interrupted. Fracture is imperfect conchoidal.

The lustre is imperfect metallic, if any thing inclining to resinous; the colour black, verging upon green or brown: the colour of the powder or streak is greenish-grey, also a little brownish. It is opake, only the edges of very thin splinters are somewhat translucent, and of a dark yellowish-brown colour.

The substance of Allanite is brittle; the hardness = 6.0; some varieties a little higher, others a little lower, but the difference is scarcely perceptible. The specific gravity is differently stated; but the highest obtained by Count BOURNON, which seems to be that of the pure mineral, is = 4.001. It does not act upon the magnetic needle.

This description does not differ in any material point from that which has been given by Mr ALLAN, except in respect to the regular forms, which were obtained from specimens discovered after the publication of his paper.

II. FERGUSONITE.

The regular form of this mineral is one of the most interesting of those comprised within the pyramidal system. The fundamental form, as deduced from approximate measurements, is an isosceles four-sided pyramid, having its terminal edges = 100° 28′, and its lateral edges = 128° 27′, Fig. 7. The character of its combinations is hemi-pyramidal, like that of Tungstate of Lime, the pyramidal Scheelium-baryte of MOHS, with which it agrees also very nearly in its angles. The character of its combinations is evident from Fig. 3., where the fundamental pyramid is in combination with an acute four-sided pyramid, with a rectangular four-sided prism, and with the pyramid of infinitely small axis, or $P - \infty$. The angle *a b c* in Fig. 6., which is the horizontal projection of Fig. 3., being about 11°, it follows that the prism *r* itself consists of the alternating faces of that eight-sided prism, whose transverse section is 112° 37′ 12″, and 157° 22′ 48″, and whose crystallographic sign, according to the method of Professor MOHS, is $\frac{[(P+\infty)^5]}{2}$. The faces of the pyramid are generally curved, and yield on that account various measures with the common goniometer, giving the inclination at its base from 158°

to 170°. The angle of 159° 2′ corresponds to the pyramid $(P-1)^6$, exactly the same ratio as we find in the two forms of pyramidal Tin-ore, noted *s* and *z* by HAÜY. The angle of 169° 26′ belongs to $(P+1)^5$. There is no sharp edge between these faces; but wherever they are most distinctly pronounced, they seem rather to approach to the position of the first of these pyramids. The acute four-sided pyramid *z* is often in combination only with $P-\infty$, as in Fig. 5.

No crystal has yet been observed terminated on both ends. In the portion of one represented in Fig. 4., the faces of the four-sided pyramid *z* are disposed in the opposite direction from that in Fig. 3. This may be conceived to be the opposite apex of the crystalline forms.

There are faint traces of a cleavage observable parallel to P, the fundamental pyramid of the species; but they are incoherent, and much interrupted by conchoidal fracture, which is of a high degree of perfection. The surface of all the forms is rather uneven, often irregularly streaked and rough.

Fergusonite possesses an imperfect metallic lustre, inclining to vitreous in the perfect conchoidal fracture. Its colour is dark brownish-black; but, in very thin scales, it appears of a pale liver-brown or yellowish-brown colour, and is translucent; in larger crystals it becomes opake. Its streak is a pale brown powder, exactly the same as in peritomous Titanium-ore.

It is brittle, the hardness = 5.5...6.0, nearer the latter; it is scarcely different from that of the prismatic Feldspar. The specific gravity, taken with great care by Dr TURNER, was found = 5.800, nearly agreeing with 5.838, the result obtained by Mr ALLAN. It does not act upon the magnetic needle.

Both these species were discovered in Greenland by Sir CHARLES GIESÈCKE′. Allanite occurs at Alluk near the south-

ern extremity of East Greenland, and, besides Zircon, it is associated with Mica and Albite, and imbedded in Quartz. The locality of Fergusonite is Kikertaursak, near Cape Farewell, where it is found in imbedded groups and single crystals, in white quartz. The specimens of this species were brought from Greenland by Sir CHARLES himself, and were presented by him, on his arrival in this country, to Mr ALLAN.

Fergusonite, under the name of Allanite, has been examined by Mr CHILDREN before the blowpipe *. The results of his experiments agree pretty well with those given by BERZELIUS of the Black Yttro-tantalite from Ytterby. Its slow but perfect solubility in salt of phosphorus, leaving a long time some particles undissolved, the property of this glass-globule to become opake by flaming, when saturated to a certain degree, and, on cooling, when still farther saturated, may indicate the presence of tantalum and yttria; and even the rose colour, which it assumes under certain circumstances, accords with the re-action of a small quantity of wolfram, as quoted by BERZELIUS. It will depend upon future examination of its physical properties, whether the black Yttro-tantalite can be arranged among the varieties of Fergusonite. They agree in some respects, but their differences in others are such, as, if ascertained with sufficient exactness, would alone prove the two substances to be different species; while the circumstance, that we know nothing of the regular forms of the black Yttro-tantalite, renders every conjecture doubtful.

It is the more necessary to leave the decision of this point to future observations, as there are some crystallised specimens in Mr ALLAN's collection, evidently belonging to a different species

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* BERZELIUS *on the Blowpipe*, translated by J. G. CHILDREN, p. 291.

from Fergusonite, and which Mr Nordenskiöld ascertained to be an yttro-tantalite. This substance is crystallised in regular octahedrons, the largest of them half a line in diameter, presenting no cleavage, but rather perfect conchoidal fracture. Its lustre is resinous, almost imperfect metallic, at least in the fracture; for the surface of the crystals, though very even, possesses but little lustre, the colour is pale yellowish-brown, the streak still lighter, almost of the same colour as that of Fergusonite, and it is translucent on the edges. It is brittle, but the hardness inferior to that of Fergusonite, being only between 4.5 and 5.0.

The locality of this species is Godhavn in Greenland, where it is found imbedded in Albite, with cleavable octahedral Iron-ore and Fluor. It resembles the yellow Yttro-tantalite of Berzelius; but the forms of this substance not being known, it is as impossible to say whether they belong to one species, as it is with other varieties of Yttro-tantalites and the Fergusonite.

Several of the properties of Fergusonite have been quoted by Mr Phillips among the characters of Yttro-columbite, and something analogous to its form is represented as a crystal of Allanite. The chief difference of this figure from the real crystals of Fergusonite is, that the faces *s*, which appear on their extremities, are inclined either to the right or to the left of each face of the four-sided prism, while in the figure quoted above, they are replaced by an eight-sided pyramid, appearing with the full number of its faces.

The mineral from Bastnaes, called *Cerite* by Hisinger and Berzelius, possesses many properties in common with Allanite. On account of its agreement in regard to chemical composition, it has been supposed by many mineralogists to constitute with it but one and the same species. It would be too precipitate to form at present a decided opinion on this question, as long as the

regular forms of Cerite are unknown, or, at least, as long as they have not been compared with those of Allanite. Professor MITSCHERLICH, however, informed me, that he had a crystal of Cerite in his possession, which the crystal of Allanite, described above, immediately recalled to his memory, when he was looking over that department of Mr ALLAN's collection which contains the minerals belonging to the order Ore.

XX. *Biographical Account of* ALEXANDER WILSON, *M. D. late Professor of Practical Astronomy in Glasgow.* By the late PATRICK WILSON, A. M. Professor of Practical Astronomy in the University of Glasgow *.

(Read February 2. 1789.)

ALEXANDER WILSON, M. D., late Professor of Practical Astronomy in Glasgow College, was a younger son of PATRICK WILSON, town-clerk of St Andrew's, and was born there in 1714. He vas very young when his father died, and was afterwards brought up by the care of his mother, CLARA FAIRFOUL, a person much respected for her prudence, virtue, and piety.

Having received the usual education at the different schools, he entered to the College of St Andrew's, where he made great proficiency in literature and the sciences, and, after completing a regular course of studies, was admitted to the degree of Master of Arts in his 19th year.

Before the expiration of his academical course, his inclination led him to prefer the study of Natural Philosophy, and particularly those branches of it which relate to Optics and Astronomy. From his earliest years he discovered a strong propensity to several ingenious arts, among which may be mentioned drawing, modelling of figures, and engraving upon copperplate. Even when a boy, he often devoted his leisure to such employments,

* This Memoir of Dr WILSON, after being read at the Royal Society, was withdrawn by its author, for the purpose of making some alterations upon it; and was never returned for publication. It was found, however, among the papers of Mr PATRICK WILSON, and is now printed with the consent of his family.

and though in all of them he was almost entirely self-directed and self-taught, yet, from time to time, he produced specimens of ingenuity which drew upon him a general attention, and which, by real judges, were considered as indications of uncommon natural talents.

Upon his leaving the College, he was put as an apprentice to a surgeon and apothecary in his native city, with a view of following that profession. At this period he became more particularly known to Dr Thomas Simson, Professor of Medicine in the University, who ever after treated him with much kindness and friendship. About the same time he had also the good fortune to find a patron in Dr George Martine, a physician in the town. In those days the construction and graduation of thermometers was little attended to or understood in Britain, and Dr Martine, from a just conception of the importance of this instrument, in many philosophical pursuits, was then employed in composing those Essays on the subject of Heat which have rendered his name so justly celebrated. The author, besides illustrating so well the theory of the thermometer, was farther very desirous of bringing accurate thermometers into general use; and, with this view, he turned the attention of his friend Mr Wilson to the art of working in glass. Though this was to him entirely a new attempt, depending upon many trials, and much mechanical address, yet he very soon acquired an admirable dexterity in forming the different parts of the instrument by the lamp and blowpipe, and in constructing and graduating the scales with accuracy and elegance; an employment which, for a long time, Mr Wilson continued to be fond of at convenient seasons, and in which it is well known he greatly excelled.

Possessing naturally much activity of mind, and employing most of his leisure in some ingenious attempt or other, it was about this time that, in making certain optical experiments, he

discovered the principles of the Solar Microscope, so far as to exhibit to several of his friends, in a dark chamber, the images of small objects enormously magnified, by the sun's rays entering at a hole in the window-shutter, and after several refractions falling upon a white ground within. But Mr WILSON as yet was too far separated from the great world, and had too little experience, for bringing forward to the notice of the public any novelty of this kind; and, soon after, a similar combination of glasses, with additional improvements, occurred to Mr LIEBERKUHN, and was at length received as a very curious enlargement of the optical apparatus.

It was also, whilst employing himself in such researches, that Mr WILSON proposed to many of his philosophical friends the idea of burning at a great distance, by means of plain mirrors, so situated as to throw the rays of the sun upon the same area, without the smallest knowledge of such a thing ever having been imagined by any person before him. But wanting the means of providing himself with any costly apparatus, the matter was pursued no farther; and it is well known that M. DE BUFFON, some years afterwards, when equally uninformed of what KIRCHER had thought of, hit upon the same conception. In 1747, by a magnificent construction far beyond the reach of Mr WILSON's finances, the French philosopher shewed what might be done in this way, and with such effect, as to render the famous secret imputed to ARCHIMEDES, of setting on fire the Roman galleys, much less apocryphal than it had ever been considered before his time.

In 1737 Mr WILSON departed from St Andrew's, and, by the advice of his friends, went to London, in order to seek for employment as a young person who had been bred to the medical profession. Soon after his arrival there, he engaged himself with a French refugee, a surgeon and apothecary of good character, who received him into his family, giving him the charge of his shop,

and of some of his patients, with a small annual salary. About twelve months after he had been fixed in this new situation, Mr David Gregory, Professor of Mathematics at St Andrew's, coming to London, introduced him to Dr Charles Stewart, physician to Archibald Duke of Argyle, then Lord Isla. Dr Stewart received him with great kindness, and, not long after, made him known to Lord Isla, who, very soon, was pleased to bestow upon him marks of his attention and favour. In his interviews with this nobleman, Mr Wilson had his curiosity much gratified by some valuable astronomical and physical apparatus which his Lordship had got constructed for himself, and had placed in his library. On the other hand, Mr Wilson was happy in being able to contribute in some degree to the amusement of his patron, by constructing thermometers of different kinds for him and his friends, with more perfection and elegance than had been hitherto done at London.

Near eighteen months elapsed in this way, during which time he conciliated the good-will and esteem of his master, by a faithful and regular discharge of whatever business was committed to his care; and, in return, he found himself now and then indulged in opportunities of keeping up his connections with persons of a philosophical cast, when his attendance upon the shop or patients could be conveniently dispensed with. Mr Wilson has been often heard to speak of the satisfaction he enjoyed even at this period, and of his perfect contentment with every thing which had then fallen to his lot. But a serenity of temper, and a felicity of disposition, were qualities which eminently distinguished him throughout his whole life.

While he thus passed his time in what he considered as a comfortable settlement at his first entering upon the world, a circumstance of a very accidental nature occurred, which gave a new direction to his genius, and which, in the end, led him to an entire change of his profession. This was a transient visit which

he happened one day to make to a letter-foundery, along with a friend who wanted to purchase some printing-types. In the course of seeing the common operations of the workmen usually shewn to strangers, he was much captivated with the curious contrivances made use of in that business. Some short while afterwards, when reflecting upon what had been shewn in the letter-foundery, he was led to imagine that a certain great improvement of the art might possibly be effected, and of a kind, too, that, if successfully accomplished, promised to reward the inventor with considerable emolument. His ideas upon that subject he presently imparted to a friend a little older than himself, who had also come from St Andrew's, and who was possessed of a considerable share of ingenuity, constancy, and enterprise. The consequence of this was, a resolution on the part of both these young adventurers to relinquish, as soon as it could be done with propriety, all other pursuits, and unite their exertions in prosecuting the business of letter-founding upon an improved plan.

It was not long ere they were enabled to carry into effect this resolution, and they first established a small type-foundery at St Andrew's, and one on a larger scale, two years afterwards, at Camlachie, a village near Glasgow.

In this situation, Mr Wilson had contracted habits of intimacy and friendship with several persons of the most respectable character, particularly with the Professors belonging to the University of Glasgow, and with Messrs Robert and Andrew Foulis, University printers. The growing reputation of the University Press, conducted by these gentlemen, gave additional scope to Mr Wilson to exert his abilities in constructing their types, and being now left entirely to follow his own judgment and taste, his talents as an artist became every year more conspicuous. When the design was formed by the gentlemen of

the University, together with Messrs Foulis, to print splendid editions of the Greek classics, he, with great alacrity, undertook to execute new types, upon a model highly improved. This he accomplished, at an expence of time and labour which could not be recompensed by any profits arising from the sale of the types themselves. Such disinterested zeal for the honour of the University Press, was, however, upon this occasion, so well understood, as to induce the University, in the preface to the folio Homer, to mention Mr Wilson in terms as honourable to him as they were just.

Though he thus continued to prosecute letter-founding as his chief business, yet, from his great temperance, domestic habits, and activity, he was enabled now and then to command intervals of leisure, which he never failed to fill up by some useful or ingenious employment. One of these, in which he took great delight, was the constructing of reflecting telescopes, an art which he cultivated with unwearied attention, and in the end with much success.

Among the more advanced students, who, in the years 1748 and 1749, attended the lectures on Divinity in the University, was Mr Thomas Melvill, so well knwn by his mathematical talents, and by those fine specimens of genius which are to be found in his posthumous papers, published in the second volume of the Edinburgh Essays, Physical and Literary. With this young person Mr Wilson then lived in the closest intimacy. Of several philosophical schemes which occurred to them in their social hours, Mr Wilson proposed one, which was to explore the temperature of the atmosphere in the higher regions, by raising a number of paper kites, one above another, upon the same line, with thermometers appended to those that were to be most elevated. Though they expected, in general, that kites thus connected might be raised to an unusual height, still they were

somewhat uncertain how far the thing might succeed upon trial. But the thought being quite new to them, and the purpose to be gained of some importance, they began to prepare for the experiment in the spring of 1749 *.

Mr Wilson's house at Camlachie was the scene of all the little bustle which now became necessary; and both Mr Melvill and he, alike dexterous in the use of their hands, found much amusement in going through the preliminary work, till, at last, they finished half-a-dozen large paper-kites, from four to seven feet in height, upon the strongest, and, at the same time, upon the slightest construction the materials would admit of. They had also been careful, in giving orders, early, for a very considerable quantity of line, to be spun of such different sizes and strength, as they judged would best answer their purpose; so that one fine day, about the middle of July, when favoured by a gentle steady breeze, they brought out their whole apparatus into an adjoining field, amidst a numerous company, consisting of their friends and others, whom the rumour of this new and ingenious project had drawn from the town.

They began with raising the smallest kite, which, being exactly balanced, soon mounted steadily to its utmost limit, carrying up a line very slender, but of a strength sufficient to command it. In the mean time, the second kite was made ready. Two assistants supported it between them in a sloping direction, with its breast to the wind, and with its tail laid out evenly upon the ground behind, whilst a third person, holding part of its line tight in his hand, stood at a good distance directly in front. Things being so ordered, the extremity of the line belonging to the kite already in the air, was hooked to a loop at

* As no public notice has hitherto been taken of this matter, though Mr Wilson had always some thoughts of doing so, it is hoped the following detail will not prove unacceptable or tedious to the reader.

the back of the second, which being now let go, mounted very superbly, and in a little time also took up as much line as could be supported with advantage; thereby allowing its companion to soar to an elevation proportionally higher.

Upon launching these kites according to the method which had been projected, and affording them abundance of proper line, the uppermost one ascended to an amazing height, disappearing at times among the white summer clouds, whilst all the rest, in a series, formed with it, in the air below, such a lofty scale, and that, too, affected by such regular and conspiring motions, as at once changed a boyish pastime into a spectacle which greatly interested every beholder. The pressure of the breeze upon so many surfaces communicating with one another, was found too powerful for a single person to withstand, when contending with the undermost strong line, and it became therefore necessary to keep the mastery over the kites by other means.

This species of aërial machinery answering so well, Mr WILSON and Mr MELVILL employed it several times during that and the following summer, in pursuing those atmospherical experiments for which the kites had been originally intended. To obtain the information they wanted, they contrived that thermometers, properly secured, and having bushy tossels of paper tied to them, should be let fall at stated periods from some of the higher kites; which was accomplished by the gradual singeing of a match-line.

When engaged in these experiments, though now and then they communicated immediately with the clouds, yet, as this happened always in fine dry weather, no symptoms whatever of an electrical nature came under their observation. The sublime analysis of the thunder-bolt, and of the electricity of the atmosphere, lay yet entirely undiscovered, and was reserved two years longer for the sagacity of the celebrated Dr FRANKLIN. In a

letter from Mr MELVILL to Mr WILSON, dated at Geneva, 21st April 1753, we find, among several other particulars, his curiosity highly excited by the fame of the Philadelphian experiment; and a great ardour expressed for prosecuting such researches by the advantage of their combined kites. But, in the December following, this beloved companion of Mr WILSON was removed by death,—to the vast loss of science, and to the unspeakable regret of all who knew him.

In the year 1752, Mr WILSON, who had married JEAN SHARP, daughter of WILLIAM SHARP, a reputable merchant at St Andrew's, brought his family to Glasgow. About five years afterwards he invented the Hydrostatical Glass-bubbles, for determining the strength of spiritous liquors of all kinds, which long experience, especially among the distillers and merchants in the West Indies, has now shewn to be more accurate and more commodious than the instruments formerly used. From the minutes of a Philosophical and Literary Society, composed of the Professors and some of their friends, whose meetings were held weekly within the College, it appears that these hydrostatical bubbles made the subject of a discourse delivered by Mr WILSON in the winter of 1757. At this time he also shewed how a single glass bubble may serve for estimating very small differences of specific gravity of fluids of the same kind, such as water taken from different springs, or the like. This he did by varying the temperature of such fluids, till the same bubble, when immersed, became stationary at every trial, and then expressing the differences of their specific gravity, by degrees of the thermometer, the value of which can be computed and stated in the usual manner.

In the year 1758 he read another discourse to the same society, upon the motion of pendulums. On this occasion he exhibited a spring-clock of a small compass, which beat seconds by means of a new pendulum he had contrived, upon the principle of the balance, whose centres of oscillation and motion were very

near to one another. At one of the trials, it performed so well as not to vary more than a second in about forty hours, when compared with a very exact astronomical clock near to which it was placed. It was some view of rendering much more simple and cheap the machinery of ordinary movements, by the slow vibrations of such a pendulum, which induced Mr Wilson to prosecute these experiments.

Not long after this, he also put in execution a remarkable improvement of the thermometer, which consists in having the capillary bore drawn very much of an elliptical form, instead of being round. By this means the thread of quicksilver upon the scale presents itself broad, and much more visible than it does in a cylindrical bore of the same capacity. The difficulty of constructing thermometers of this kind, had nearly hindered him from completing his invention, as the thread of quicksilver was found extremely liable to disunite when descending suddenly in so strait a channel. But, by his long experience, joined to farther investigation, and more trials, he at last discovered a method of blowing and filling thermometers with flattened bores, which freed them entirely from this defect.

About the same time, also, he conceived the design of converting a thermometer graduated for the heat of boiling-water, into a Marine Barometer, in consequence of the well-known difference of temperature which water, when boiling, acquires under the variable pressure of the atmosphere. This he effected, by making a boiling-water thermometer, about a foot in length, with a pretty large ball, and having a thread of quicksilver as broad and visible as was consistent with a very perceptible run upon small alterations of temperature. The stem of this thermometer he fortified, by inclosing it in a cylindrical case of white iron, having soldered to it, at its lower end, a socket of brass for receiving half of the ball, which afterwards became entirely de-

fended, by screwing to the socket a hemispherical cap. At the other end of the case which environed the stem, there was soldered a tube of brass, wide enough to admit a scale of proper dimensions, before which there was an opening in the tube, defended by glass.

The utmost range of the scale he determined by the points, where the thermometer was found to be stationary when the ball and a certain part of the stem were immersed in water, boiling under the greatest variations of pressure which the climate afforded. The interval so found, he subdivided by other observations into degrees, which corresponded to *inches* of the barometer, and which were so denominated upon the scale.

In the year 1756, the College of Glasgow, upon the death of Dr Alexander Macfarlane of Jamaica, a great lover of, and proficient, in the sciences, received a legacy of a valuable collection of astronomical instruments, which that gentleman had got constructed at London by the best artists, and had carried out with him to Jamaica, with a view of cultivating astronomy in that island. The College, upon this, soon built an observatory for their reception, which, by medals placed under the foundation, was called by the name of their generous benefactor; and Mr Wilson was immediately thought of by the members of the Faculty, as a proper person for taking charge of it, and making the astronomical observations. At this juncture his Grace Archibald, Duke of Argyle, who had all along continued his patronage to Mr Wilson, more especially since he had brought the art of letter-founding into Scotland, used his influence with Government, and procured his Majesty's presentation, nominating and appointing him Professor of Practical Astronomy and Observer in the College, with an annual salary of fifty pounds, payable out of the Exchequer; and, accordingly, in 1760, he was admitted to this new office by the unanimous and most cordial welcome of all the members of the Faculty.

His two eldest sons, who had by this time entered upon a course of liberal education, not long after took upon them the further enlargement and improvement of the letter-foundery; and, before dismissing this topic, it deserves to be mentioned, that Mr Wilson lived to such an advanced age, as to enjoy in the most feeling manner the reward of his early diligence and excellent example, in seeing the business rising in their hands to the highest reputation, not only in these kingdoms, but in foreign countries.

In 1763, when upon a visit at St Andrew's, an honorary degree in medicine was conferred upon him by his Alma Mater.

Among the objects which now occupied him in the Observatory, his former labours towards improving the reflecting telescope were resumed, and pursued for a considerable length of time, with a view of obtaining some certain method of giving the parabolic figure to the great speculum. These trials were made upon a variety of metals, comparatively of a small diameter, and focal distance; but he regarded them only as preliminary ones, and had always in contemplation to engage with apertures of much greater dimensions. He was often heard to regret, that no crowned head, or wealthy association, ever thought of patronising an attempt to construct some vast telescope, to be employed in making discoveries in the Moon or Planets, or in exploring the Heavens; and, it is more than probable, that if his own means had been less circumscribed, he would of himself have attempted something of this kind. The more recent labours, and brilliant success, of the excellent Dr Herschel, have fully shewn that such suggestions were by no means romantic; and the writer of this account, who has had the happiness of being well acquainted with both these men, has often remarked a striking resemblance in their character and turn of mind.

In 1769, Dr Wilson made that discovery concerning the solar spots, of which he has treated in the Philosophical Transac-

tions of London for 1774. Not long after he entered upon this new field, the nature of the solar spots was announced by the Royal Society of Copenhagen as the subject of a prize essay. This induced him to transmit thither a paper written in the Latin language, containing an account of his observations, and of the conclusions drawn from them. In return, he obtained the honourable distinction of a gold medal of near sixteen guineas intrinsic value, having, on its reverse, the figure of Truth pendent in the air, holding a wreath in one hand, and in the other a perspective glass, and the motto, *Veritati luciferæ*.

As an astronomical observer, he was remarkable for a sharp and clear eye, devoid of all blemish, and which, too, without being liable to fatigue, had long been inured to examine and to judge of small objects in their nicest proportions; a circumstance which must have proved of great advantage to him, when employing his sight upon celestial appearances by means of the telescope; and it required only to know him, to have the fullest assurance of his fidelity in rendering an account of his observations.

His discovery in regard to the solar spots, though it be gaining ground more and more among those most conversant in astronomy, yet, like many other new discoveries, has not escaped its share of opposition. This gave him occasion to publish, in the Philosophical Transactions of London for 1783, the second paper upon that subject, after a silence of near ten years, wherein, upon the authority of many more observations made in that interval, he obviates objections, and maintains the reality of his discovery, with an entire conviction. The amount of it is, "That the spots are *cavities* or *depressions* in that immensely resplendent substance which invests the body of the sun to a certain depth; that the dark nucleus of the spot is at the bottom of this excavation, which commonly extends downwards to a space equal to the semidiameter of our globe; that the shady or dusky zone

which surrounds the nucleus, is nothing but the sloping sides of the excavation reaching from the sun's general surface downward to the nucleus or bottom." All this he has demonstrated by a strict induction drawn from the following phases of the spots, as they traverse the sun's disk.

When a large well-formed spot, consisting of a dark nucleus, and its surrounding umbra or dusky zone, is seen upon the middle of the sun's disk, the zone is generally equally broad all around; but when the same spot verges near to the limb, that side of the dusky zone which lies next to the centre of the disk, begins much sooner than the side diametrically opposite to turn narrower, and at last disappears, while the other still remains dilated and visible. And, in like manner, when a spot enters the disk, by the sun's rotation, we see first the nucleus, and the upper and under sides of the shady zone or umbra, together with that side of it nearest to the limb, whilst the side opposite is still wholly invisible. But as the spot advances farther upon the disk, that side of its dusky zone which lately was invisible, now shews itself, and continues to enlarge more and more, till it becomes as broad as any other part surrounding the nucleus.

These phases, which he found so very palpable when observing carefully the great solar spot in November 1769, and so very frequent, though less obvious, in numberless other spots of a smaller size, which for several years afterwards he examined, prove in the clearest manner that the spots themselves are depressions in the luminous matter of the sun, and lead to many new and interesting ideas concerning the nature and constitution of that stupendous body.

But though he was the first astronomer to whose lot it fell to remark these phenomena of the solar spots which have been just now described, and to draw such important conclusions from them, it appears that the celebrated Mr Flamstead, so far back as the year 1676, had very nearly anticipated this discovery.

For, one day when observing a spot of considerable size near the sun's limb, he actually beheld this appearance of the dusky zone which belongs to the nucleus, finding it almost wholly deficient on that side which respected the centre of the disk; and this, too, when the distance of the spot from the limb corresponded very nearly with that which Dr Wilson found to be so constant in his observations. Mr Flamstead was then, indeed, viewing his spot in peculiar circumstances, and the most favourable of all to perfect vision of the sun, as, by the intervention of a mist, he was enabled to use his telescope without the help of tinged glass put before his eye. The following is his account of this remarkable observation, in which, by the word *macula*, Mr Flamstead evidently means the nucleus of the spot, and by *nubicula* the dusky zone which surrounds it.

"1676, Nov. 9. Deinde densi adeò vapores excepere solem, ut per ipsos licuit illum nudis oculis intueri. Adhibito tum longiore tubo absque vitro rubro, (quo oculum adversus ejus splendorem munire soleo) maculum contemplatus sum: distincta valdè videbatur, ejusque figuræ quæ in schemate adpingitur: 'Nubecula ipsi circumducta elliptica omninò; *sed, quod valdè miratus sum,* admodum dilatata à parte limbum respiciente; ab altera vero versus centrum, maculæ fere cohærere videbatur.'

"Observavi dein maculæ a limbo proximo distantium 1′ 13″." *Hist. Cœlest. Flamsteedii*, vol. prim. p. 363.

When Dr Wilson saw the great spot on the 23d November 1769, it had nearly the same situation upon the disk, and the same aspect as the one here described. But, at that time, like Mr Flamstead, he had no conception of what was signified by such an appearance. It was not till next day, after remarking certain striking alterations of the form both of the nucleus and umbra, that the suggestion first arose in his mind of the spot being an *excavation* or *depression* on the luminous matter of the

sun; which idea, the subsequent observations of the same spot most evidently confirmed.

Not long before his death, in turning over at more leisure the pages of this admirable astronomer, Dr WILSON, for the first time, met with the above passage, and was pleased at finding so remarkable a coincidence as to the leading fact upon which his discovery rests.

Among his papers there were found many letters he had received from Dr MASKELYNE, upon whose correspondence Dr WILSON set a very high value. All his papers, published in the Philosophical Transactions of London, were communicated by that friend. Among these, we find a short one in the volume for 1774, wherein he proposes to diminish the diameter of the finest wires, used in the focus of the astronomical telescope, by flattening them according to a method there described; an idea which, though very simple, seems extremely worthy of attention.

In the month of January 1777, when conversing, as he often did in the evenings, with his son, who had now made some proficiency in the sciences, their attention was somehow turned to the following query, proposed by Sir ISAAC NEWTON, among many others, at the end of his Optics, namely, "What hinders the fixed stars from falling upon one another?"

In reflecting upon this matter, they readily came to be of opinion, that, if a similar question had been put in respect of the component parts of the solar system, it would have admitted of a very easy solution, on account of *periodical motion* appearing to them as the great mean employed by nature for counteracting the power of gravity, and for maintaining the sun and the whole retinue of planets, primary as well as secondary, and of comets, at commodious distances from one another.

In like manner, Dr WILSON thought it not unreasonable to suppose, that the same principle might have assigned to it a do-

minion incomparably wider in extent, and that the order and stability, even of a *universe*, and of every individual system comprehended in it, might depend upon *periodical motion* round some grand centre of general gravitation. This conception, besides appearing to them warranted by every view they could take of the nature of gravity, seemed moreover to receive some support from the discoveries which, since the time of the great HALEY, have been made of what has been called the "Proper motions of the fixed stars," and particularly from the opinion entertained by that excellent astronomer Dr MASKELYNE, "That, probably, all the stars are continually changing their places by some slow and peculiar motions throughout the mundane space."

Soon after this view had arisen, out of the familiar conversation above mentioned, it was published in a very short anonymous tract, entitled, "Thoughts on general Gravitation, and Views thence arising as to the state of the Universe." The chief inducement to so early a publication, was the hope of drawing immediate attention to so interesting a point, which might possibly lead to the discovery of some way by which the matter might be brought to the test of observation.

It is quite obvious, that the foregoing suggestions necessarily imply a motion of the solar system, as one of that immense host, which, for what we yet know, may be subjected to the laws of periodical revolution. Accordingly, it early occurred, that, perhaps the most advantageous way of advancing in this investigation, might be to try to find out, if possible, symptoms of such a law as affecting that system to which we ourselves belong.

It sometimes struck him, when looking over the progress of philosophical discovery, that many things of high moment appear to have lain long wrapped up in embryo, by our not employing ourselves more frequently in what may be called "a *direct search*," and in filling up with more attention and boldness the list of de-

siderata. Between this last step, and the accomplishment of a profound discovery, he conceived that the transition might sometimes be made with no great effort of invention, by only sifting carefully such principles as are already known and familiar to us, and availing ourselves of them in their full extent.

It was by proceeding in this way, and, when considering the manner by which the motion of light would be affected by reflecting and refracting media, themselves moving with great velocity, (a most interesting field in Optics, then wholly uncultivated), that two principles came into view, either of which may possibly serve us in detecting a general motion belonging to the solar system, relatively to the surrounding fixed stars, or in proving a negative with regard to it. Of these, a very summary account has been given in the historical part of the Edinburgh Philosophical Transactions, vol. i. But, should they be successful in discovering such a concealed motion, the same principles cannot fail of determining the velocity and direction of it; and, in process of time, whether such a translation of the whole system be in a straight line or a curve, and if in a curve, whether it be of such a kind as may indicate a periodical revolution. And it needs scarce be mentioned, that if such a thing should actually be made out, besides enriching astronomy with that knowledge which depends upon measurable parallaxes in the sphere of the starry firmament; it would also bestow a very high authority upon Dr WILSON's suggestions, of what possibly may be the plan of Nature in upholding the universe.

At the time of the last-mentioned publication, he was sixty-three years old, but still continued to enjoy the blessings of an uninterrupted state of good health. In the year 1784, at the recommendation of the University, his Majesty was graciously pleased to nominate and appoint PATRICK WILSON, A. M., Dr WILSON's second son, to be assistant and successor to his father, as Professor of Practical Astronomy and Observer; a circum-

stance which heightened the consolations he enjoyed during the evening of life.

In March and April 1786, when he had nearly completed his seventy-second year, it became apparent to his family and friends that his constitution and strength were fast declining. After a gradual and easy decay, which lasted throughout the whole of that summer and autumn, and which he bore with the utmost composure and resignation, amidst the tender solicitudes of his surrounding family, he at last expired in their arms, on the 16th day of October.

The private character of Dr Wilson was amiable to an uncommon degree. From his early youth to venerable age he was actuated by a rational and stedfast piety, enlivened by those gracious assurances which carry our hopes and prospects beyond the grave, and sweeten the lot of human life. The cast of his temper, though uniformly cheerful and serene, was yet meek and humble, and his affections flowed in the warmest current immediately from the heart. His looks, as well as his conversation and demeanour, constantly indicated a soul full of innocence and benignity, in harmony with itself, and aspiring to be so with all around it.

XXI. *On the Determination of the Species, in Mineralogy, according to the Principles of Professor* Mohs. By William Haidinger, Esq. F. R. S. Edin.

(Read November 15. 1824.*)*

A justly celebrated Naturalist * was of opinion, " that every " distribution of mineral bodies, which is instituted before the " determination of the species, must be mere confusion; while, " after having established it, according to fixed principles, no " kind of distribution can be absolutely faulty." The distribution itself varies, along with the different principles of classification, introduced for the purpose of obtaining a systematic arrangement, conformable to the views of Natural History, of Chemistry, or of other sciences; but the Species remains that unique and unalterable point to which every system, and in fact every inquiry, must be referred, if we wish to avail ourselves of the prerogatives of the human mind, and preserve our information in a scientific form.

The correct determination of the species is equally important to every branch of our knowledge, in regard to the objects of nature; since there does not exist another general idea applicable to the same extent, or equally fertile in producing order and stability, within the daily increasing mass of observations. It is

* Dolomieu, *Sur la Philosophie Minéralogique*, p. 113. Avant d'avoir préalablement fondé l'espèce, toute distribution n'est que confusion; après l'avoir établie sur des principes fixes, aucune distribution ne peut être, jusqu' à un certain point, vicieuse, parce qu'elle a toujours un fanal qui l'éclaire, un point de rappel d'où partent toutes les relations, et auquel toutes doivent concourir.

the foundation of scientific Mineralogy, and it must, on that account, be reduced to constant and philosophical principles, and not be left at the mercy of chance and empiricism.

If we consider the progress of mineralogical systems from the first attempts of Linnæus to the present day, it appears that, while the general scientific form of the systems was insensibly lost,—while essential degrees of classification were abandoned,—while even the characteristic marks between substances resembling each other were deemed superfluous *; the determination of the species continued gradually to approach nearer to perfection. The system of Linnæus, with its ternary distribution, containing three classes, each of which comprises three orders, and the system of Wallerius, with its quaternary distribution, each of his four classes comprehending four orders, gave way to the systems of Cronstedt and Werner, in which the characteristic differences, and the order, as a unity of classification, disappeared, and to the system of Haüy, in which distinctive characters are introduced, not for discriminating a newly observed individual from all the rest, but for producing a contrast between it and one or a few others, mentioned by name, and which, moreover, at least in one, and the most numerous of its classes, contains neither orders nor genera.

However valuable the labours of Werner may have been in clearing the mineral system of compound rocks formerly held, along with the simple minerals, to be proper objects of mineralogy, yet it is particularly to the accurate investigations of the forms by Haüy, resting, in part, upon Romé de L'Isle's observations, that we are indebted for a more correct determination of the species contained in his system. This greater degree of correct-

* Linnæus, when speaking of the system and principles of Cronstedt, says, "*Definitiones characteristicas inutiles judicat: sufficere nosse.*"—Syst. Nat.

ness has been rather the consequence of a more intimate acquaintance with nature, than the result of applying philosophical principles to observation. The application of geometry in the consideration of forms, imparted precision to a property which had hitherto been as vague and uncertain as others The striking contrasts offered by the forms of different species being once recognised, it became almost impossible for HAÜY, and subsequent crystallographers, to fall into the same errors which render the systems of an earlier period useless. The crystallographic method of WERNER was far from affording precision and security, in its determinations; and his species, therefore, are less correctly circumscribed than those of HAÜY, who, notwithstanding the great superiority of geometrical evidence to that of mere inspection, considered the introduction of a chemical principle into the determination of the species as unavoidable. Although chemistry has always exercised a great influence upon the methods received in mineralogy, it cannot be said that this was more particularly the case in respect to the establishment of the idea of the species, when it even required the sagacity of ROMÉ DE L'ISLE to demonstrate that there really were such things as true species in the mineral kingdom, which till then was denied by the chemists of the day. The Wernerian System demonstrates, in every one of its departments, that it has in fact no principle upon which it might be said solely to depend; and the characteristic ingredient itself may be ambiguously interpreted. This want of unity in the principle of the Wernerian system, has perhaps nowhere been more plainly expressed than in the first edition of Professor JAMESON'S *System of Mineralogy*, where we find the following passage: "The Wer-" nerian oryctognostic system is founded solely on the *natural* " *alliances and differences observable among minerals.* But on " what do these depend? WERNER answers, on the quality,

"quantity, and mode of combination of the constituent parts." This is in fact tacitly supposing the results of mineralogy and chemistry already to have been brought to a degree of coincidence, which, though it may be rightly anticipated, has yet not been established, even by the recent most rapid progress of the two sciences. In this way the determination of the species of WERNER, was almost entirely left to a certain tact which admits of no definition, and must be more admired in the correct results occasionally obtained, than praised for the certainty with which the method could be pursued.

The definition of the species, as given by HAÜY, that it comprises *bodies having similar integral molecules, and being composed of the same elements, in the same proportion* *, consists of two heterogeneous parts, the first of which, taken alone, is insufficient, though appertaining to the science, while the second is altogether foreign to mineralogy.

Natural Philosophy has been defined as "the science that "unfolds those general principles which connect the events of "the material world †." As a branch of natural philosophy, the science of Chemistry regards the events produced by the action of bodies upon each other, in which the changes produced are permanent, and, therefore, like the whole of natural philosophy, it investigates phenomena in reference to their causes.

It is necessary to know what things those are, which act upon one another; and definitions must therefore precede, before it is possible to enter into the details of these sciences themselves. One word may very often suffice for the definition of one object, but if there be a great many, some of them, too, possessing very similar properties, a degree of order must be introduced, even in the enumeration of the above-mentioned definitions.

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* *Traité*, t. i. p. 162.

† LESLIE, *Elements of Natural Philosophy*, p. 1.

Confining ourselves, in the present place, entirely to the productions of Nature, there are in fact a great many bodies, which more or less resemble each other. The problem to be resolved here, as a first process to the introduction of other sciences, will be to discriminate those which resemble each other most. This discrimination is the object of Natural History; a science, therefore, which proceeds upon the principle of similarity.

Hence, it appears, that Chemistry and Mineralogy most materially differ in regard to the point of view from which they consider the productions of inorganic nature.

Mineralogy treats of those properties which minerals exhibit in their natural state; it collects the individuals within the ideas of species, genera, &c., and, teaches us how to distinguish them from one another. Chemistry refers to the substances of which the natural-historical species consist; it treats of the properties of inorganic matter, manifested during the process of its forming new combinations, and teaches us to recognise these substances, not only in their pure state, but in all their various mixtures and combinations; and it enables us, by the comparison of its results with the species previously determined, to form an opinion of the chemical constitution of the latter.

The establishment of what may be with propriety considered as a Species in Natural History, according to the pure principles of that science, is its most important object, because its whole scientific progress depends upon this idea. Mineralogists have hitherto, almost uniformly, considered it as impracticable to attempt the construction of systems, or the definition of the species, without the assistance of chemistry: nay, professed mineralogists have not hesitated to allow to the identity of chemical substance, the first and most important place among the considerations upon which they ground their species; and, owing to this preponderance, we see Arragonite and Calcareous-spar, and the hexahedral and prismatic Iron-pyrites, in recently published

systems, degraded to subspecies or varieties of carbonate of lime, and sulphuret of iron *.

These systems shew, that, in reality, they are not intended for mineralogical purposes, but for exhibiting the present state of our information respecting the chemical constitution of the mineralogical species. They endeavour to exclude, in the formation of the species, the influence of those properties which the productions of inorganic nature possess in their natural state; but, it is evident, from the want of consistency in their different parts, that they cannot arrive at their purpose, unless they admit, as their leading point of comparison, the species, determined without the slightest influence of chemical properties or considerations.

Professor Mohs has succeeded in producing a definition of this idea, founded entirely upon the comparison of those properties which may be observed while the minerals continue in their natural state; and it is the object of the present paper to point out the course, and to develope the principles, conformable to which he has arrived at this important result.

There can be no doubt, that, in Mineralogy, as well as in Zoology and Botany, the species should comprehend the assemblage of those individuals which are most closely allied to each other. The exactness of the definition will therefore depend upon the signification we attach to *individuals*, and to the degree of *alliance* necessary for joining them within one and the same species.

Natural History requires that an individual be a single body, and, as such, by itself, fit to be an object of natural-historical consideration. It implies unity of form, and does not presuppose the existence of, or connection with, another individual.

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* Beudant, *Traité élémentaire de Minéralogie*, p. 406. 425.

In Botany and Zoology, the determination of what constitutes an individual, presents little difficulty, at least in the higher classes of natural bodies. The branch of a tree is not an individual itself, but part of one, and is considered in botany as belonging to it, on account of its connection with the individual. If we draw inferences from portions of the fossil remains of animals, it is likewise only in so far as we are conscious of having observed parts only of individuals. It is more difficult to establish the same idea in respect to the class of worms, and other inferior productions of the animal, and also of the vegetable kingdom, wherever the multiplication of individuals depends not only upon the process of generation, but may be effected by dividing a whole into several parts, which, after some time, may each of them be again considered as a whole. The definition of the species is here, in a great measure, dependent upon life, which, during a certain variable period of time, places these bodies beyond the reach of the powers that affect inanimate matter, if removed from that condition. It is as essential to the organic kingdoms, as the power of crystallisation is to the products of inorganic nature. This power produces crystals which possess regularity of form, and are evidently individuals. But although this regularity should disappear, by the contact of several crystals with each other, yet the continuity of the homogeneous matter contained within them, and the unity of form, remain unimpaired, and the portions of the compound mass, which may be traced to the formation of one continuous crystallised product, still remain individuals. The individuals may be of various sizes, and often are very small. We have frequently occasion to observe granular varieties of Calcareous-spar, in which the component individuals are large enough to be disengaged from their contact with others, and their properties examined: in other varieties they are of a smaller size, though still recognisable: but they are often so small, that only a faint glimmer betrays their existence; yet,

from having observed the whole series of compound varieties, we infer that the last is exactly the same thing as the first, and only distinguished from it by the size of the individuals. It is often the case that compound minerals again consist of compound varieties, variously aggregated: thus Peastone is a granular composition of globular particles; each of them consists of concentric layers, parallel to their surface, and each of these only, if further examined, shews the disposition of delicate fibrous individuals, in a direction corresponding to that of the radii of a sphere. Compound minerals are objects of a natural-historical consideration, only in so far as they consist of individuals; in the same way as herds of cattle or forests are considered in zoology and botany, only because they consist of individuals, the more peculiar object of these sciences.

Various opinions have been current among mineralogists as to the exact meaning of the term *individual* in mineralogy. Some extended the idea of individuality to every specimen or fragment of a mineral whatever. Others maintained that perfect crystals only could be considered as individuals. According to the opinion of some, there was only one individual, the species itself; while others considered the mountain-mass from which the specimen had been detached, as the only true individual. Many denied the existence of individuals in mineralogy altogether, or fixed this idea upon the ultimate particles of matter, in which the mechanical connection cannot be resolved, without, at the same time, destroying the chemical mixture.

According to Professor MOHS, perfect crystals are individuals, but not the only ones; every product of the power of crystallisation, consisting of continuous and homogeneous matter, of a regular or irregular form, large or small, every component particle of granular limestone, every fibre of hematite, being likewise comprehended within that idea.

The connection among certain individuals, in preference to

others, in order to constitute a species, is produced by examining every property by itself, which minerals present, and joining those individuals which either possess the same properties, or in which the gradations of the differences in their characters may be considered as producing continuous series. For example, if it be impossible to unite two varieties of form, or two degrees of specific gravity, within a single series, it is evident that the individuals under consideration themselves cannot be comprehended within the limits of the same series.

The fundamental proposition upon which, in natural history, we ground this reasoning, is, that two individuals, which do not differ in any of their properties, are *identical*, and may be substituted for one another, in every inquiry carried on according to the principles of that science; so that, if we have found one of them to belong to a certain class, to a certain order, genus or species, the other individuals also must belong to the same assemblage of natural productions. Two hexahedral crystals of Fluor, possessing the same violet-blue tint of colour, the same degree of transparency, and agreeing exactly in hardness and specific gravity, may serve as examples of identical minerals.

But if, instead of one of these crystals, we compare the violet-blue hexahedron with another individual of Fluor, possessing the same form, but a honey-yellow colour, or the same colour, but the form of a regular octahedron, the two individuals will evidently be different, even though the rest of their properties should be critically the same. A difference will also exist, if, instead of the second individual, we take a hexahedron of Gold or of Rock-salt; and the difference between the latter individuals and the individuals of Fluor is no doubt much greater than that among the above-mentioned crystals of Fluor themselves. From these considerations we infer, that the difference among several individuals is not the same in every instance, but that various

bodies may differ more or less from each other in regard to their properties.

Experience shews, that, among the vast number of individuals which may in this way be compared with each other, there are some in which all the properties agree, except a single one. Thus we find hexahedrons of Fluor agreeing in every respect, except the colour, which is blue, or green, or yellow, or even sometimes perfectly white. This result of immediate observation may, however, be obtained in a much more general and satisfactory manner, by considering it in the regular forms of minerals. The combination of the hexahedron and the octahedron, in an individual of Fluor, may be considered as the product of the power of crystallisation, which caused this individual to assume at the same time the form of the hexahedron and that of the octahedron. With each of these two forms, all the rest of the properties, to be observed in the individual, must be necessarily connected. Every combination, however great the number of simple forms which it contains, serves to demonstrate this proposition. We obtain thus a number of individuals, which belong to the same series of crystallisation, and which, in regard to the rest of their properties, are absolutely identical. The preceding example shews, that if a number of individuals differ only in a single one of their properties, the differences in this property may be such as to allow them to be considered as the gradations of a continuous series. This series may be the series of crystallisation, as in the example; but it may be every series that can be possibly produced by gradations in the properties, as, for instance, in colour, in lustre, in transparency, &c.; nay, it may be extended even to those properties which remain constant in every instance, and which may be considered as producing a series, all the members of which are equal.

Every individual, not excepting those which appear in compound forms, must necessarily be identical with itself. If, in an

individual of this kind, we enlarge the faces of one of the simple forms, after the other, we obtain a series of individuals, each of which is in exactly the same relation to the idea of identity as the fundamental individual, and though they are not absolutely identical, yet they agree in respect to this idea. It is evident that the forms may be arbitrarily exchanged with each other, without in the least producing any change in respect to the idea of identity. But two or more individuals become absolutely identical, if we suppose them to possess one and the same form; and this is the process by which individuals, though not identical by themselves, may yet be brought under the idea of identity. In the example of Fluor quoted above, the hexahedron and the octahedron, both possessing the same cleavage, the same refractive and dispersive powers, the same colour, the same degree of transparency, the same hardness and specific gravity, &c., may be taken in so far for identical, as every thing that may be found to be true of the one, in respect to a more general consideration of natural history, will hold equally true in regard to the other: we are entitled to exchange the hexahedron for the octahedron, or, in general, any two members of one and the same series of crystallisation, without destroying the idea of identity.

It is rare to find a number of individuals in nature which differ only in one of their properties. More generally we meet with such as, at the same time, deviate more or less in one or several of their other properties; and, in order to be capable of drawing more general inferences, it is necessary to join several series, like those considered above, within one and the same idea. Let us suppose a number of individuals of Fluor to be compared with each other, all of them possessing the same colour, a dark violet-blue, but various regular forms, as the octahedron (Aberdeenshire), the dodecahedron (Ehrenfriedersdorf), the hexahedral trigonal-icositetrahedron (St Agnes), the second variety of tetragonal-icositetrahedrons (Zinnwald), and various combinations of the same

simple forms. With the same facility, we establish, from experience, a series of individuals, all of which appear under the form of the hexahedron, but which present various colours, as honey-yellow (Annaberg), wine-yellow (Freiberg), white (Alston), apple-green (Beeralston), sky-blue (St Agnes), azure-blue (Alston). If now we find an individual which possesses a dark violet-blue colour, and whose form is the hexahedron, like some of those varieties which have lately been found at Gourock, this individual may be considered as a member of the first series, and also, at the same time, as a member of the second series; and all the individuals constituting the one will thus be brought under the idea of identity with all the individuals of the other; so that we are now entitled to consider as identical not only those combinations of form and colour which we have really observed, but also those which may be obtained by joining arbitrarily any form, or any colour, of the two series with each other. The varieties thus produced are in fact very frequently likewise observed in nature. According to the same principle, we may extend our considerations to all those properties of minerals whose gradations form series, and include an assemblage of individuals, which, notwithstanding their differences, may yet be brought under the idea of identity. Those individuals to which the process is not applicable, are, at the same time, distinctly and accurately excluded; and the assemblage produced does not only contain every thing that can possibly be united with it, but, at the same time, every thing is distinctly excluded, which must be considered to be foreign to it, according to the principles of natural history.

These assemblages of individuals, connected by the gradations of their properties, in the same series, are designated in mineralogy by the name of *Species*. The individuals comprehended, are intimately connected, and, at the same time, distinctly separated, by these series of characters from all the others. A species may therefore be defined to be *the assemblage of indi-*

viduals, whose natural-historical properties, that is to say, those properties which may be observed while the mineral continues to exist, are either absolutely the same, or present gradations which form continuous series. These are either determinable *a priori*, from one given member, or they are the result of accurate and long continued observation. In the latter we are not permitted to venture one step beyond the boundaries of actual observation, while, in regard to form, we are entitled to suppose, according to analogy, that, whatever may be wanting for the necessary completion of the representation, may still be supplied in establishing the idea of the species.

We must not pass over unnoticed any of the characters or natural-historical properties, otherwise the idea of the species itself would be incomplete; the variety displayed in nature could not be explained sufficiently, nor could it be demonstrated that *we are really entitled to consider certain bodies, under the required circumstances, as belonging to one and the same species, although they differ essentially in some of their properties.* The properties which are most useful, and must be particularly attended to, are those which can be expressed by numbers or measurement; in fact, those which allow of the application of mathematics. The properties referring to form and cleavage, simple and double refraction, angle of maximum polarisation, and the dispersive power, hardness and specific gravity, are therefore most minutely to be examined, if we wish to arrive at a correct result. But it would be contrary to the principles of natural history to determine the idea of the species according to one single property, because, by being thus confined, we would soon fancy ourselves under the necessity of introducing considerations which are foreign to the science *.

* C'est dans ces sortes de cas où les caractères géométriques se taisent, qu'il devient nécessaire d'avoir recours à la Chimie. HAÜY, *Traité*, 2de Ed. t. iv. p. 179.

The progress of the gradations in the single properties of the individuals within one species, is what may be called with propriety a *transition* or *passage;* and individuals in which such a progress is observable, are said to *pass into each other*. The transitions arise from the series of characters; and we may infer from the existence of transitions between individuals, that they belong to one and the same species. Transitions exist only within the species, and no transition can take place from one species into another, because this very occurrence of a transition between two supposed species, would unavoidably join them into a single one. In regard to the numerous incorrect transitions mentioned in mineralogical books, Professor Mohs says, "that, "wherever the transition is correct, the determination of the species is erroneous, and *vice versa;* that the transition is falsely "stated, if the determination of the species be correct." *

The species, obtained by the process explained above, is the object, not the product of classification, as some mineralogists seem to believe, who begin and terminate classification without previously digesting the idea of species. It has been a common practice to take advantage of the constancy of the chemical composition within well defined natural-historical species, and thus to exclude, as it were, the definition of the species from the province of natural history, and to resign it entirely to the ascendancy of chemical principles. Yet the inferences drawn from chemical observations presuppose the existence of the species, as produced by the comparison of the properties observable in their natural state. One specimen of a species having been analysed, we may conclude that other specimens of the same species consist of the same principles, in the same proportion; but, we are entitled to draw this conclusion only, because the ana-

* Mohs' *Treatise on Mineralogy*, Transl. vol. i. p. 337.

lysed specimen, and all the rest, agree in their natural-historical properties, not because they are acted upon by heat, or other chemical re-agents, in a similar manner; for this would only be a repetition of the first process,—from the analysis of one specimen, (however small), to infer the chemical constitution of the rest of the individuals or varieties included in the species.

It has often been urged, that the method of Professor MOHS tended to depreciate the merits of Chemistry, by excluding its influence in the determination of the species, and in the construction of mineralogical systems. This is by no means a just observation. Every one is aware, and more particularly those who have made minerals their study, that it is chemistry, in its various departments, which gives their labours a manifold interest, in supplying the link between their abstract considerations and the wants and comforts of mankind. The only thing objected to by Mr MOHS, is that unnecessary and improper union of chemistry and mineralogy, in other words, *chemical mineralogy;* which, so far from producing the good consequences so frequently boasted of, may actually be considered as having retarded the researches directed towards the knowledge of the properties which minerals present in their natural state; when it was deemed more satisfactory to ascertain, however imperfectly, the composition of a mineral, than to measure the angles of its crystalline forms, to determine its action upon light, or obtain from experiment its hardness and specific gravity.

Fortunately for mineralogy, chemists have advanced so far in their inquiries, that, besides obtaining new compounds, and discovering new modes of producing them, they may now devote their attention to a more accurate examination of those which they have known long ago to exist. Professor MITSCHERLICH'S doctrine of isomorphous bodies, is one of the most interesting consequences already deduced from these comparisons. The path entered upon by the artificial production of Pyroxene, of

Chrysolite, and other species that have long ago been observed in nature, will no doubt lead to brilliant discoveries. It is to be expected that, the more we are capable of multiplying the points of comparison between the natural-historical properties and the chemical constitution of minerals, the more we shall find them to tend towards harmony in their results; but the correct determination of the mineral species, according to those properties which it must always possess, in its unaltered and permanent state, can alone guard us from stooping to empirical laws, when it is practicable to arrive at results as direct as geometrical demonstrations, and not inferior in evidence.

XXII. *On the Consolidation of the Strata of the Earth.* By

Sir James Hall, Bart. F. R. S. Lond. & Edin.

(Read April 4. 1825.)

The public attention, animated by scientific controversy, has of late years been much directed to Geological subjects; and the certainty of many important facts, has in consequence been ascertained beyond dispute, which were formerly unknown, or at least involved in such obscurity, that no person could have ventured to assert them, without being charged with extravagance. But though, no doubt, many branches of this science still remain to be investigated, such inquiries may now be said to have acquired a considerable degree of consistency and interest, from the substantial basis upon which they have been found to rest.

Thus, in the present day, it is universally admitted, that a great part, I believe, in point of bulk, by far the greatest part, of the solid rock which constitutes the external mass of our globe, is stratified: that these strata, or at least a considerable portion of them, have at one period consisted of a loose assemblage of sand and gravel, broken from rocks of still higher antiquity: that these fragments are infinitely various in quality, in bulk, and in form; some retaining their original sharpness, others rounded and polished by agitation in the water: that these beds alternate with others of limestone, composed, in a great measure, of the shells of sea-fish, which shells are also occasionally scattered through the other strata. So that on the whole, it seems to be ascertained to the satisfaction of all par-

ties in geology, that the strata,—those, at least, of later formation, have once constituted collections of incoherent parts. And it is further admitted, that these beds have undergone various remarkable changes, some chemical, some mechanical.

The chemical changes consist in the consolidation of these loose assemblages into their present state of rock, passing, in that transition, through boundless varieties, in point of flexibility and toughness, and occasional brittleness. The mechanical revolutions are no less remarkable, principally in the change of the strata to their present contorted shape, and elevated position, often many thousand feet above the surface of the sea; though there is full reason to believe that they all once lay in a horizontal position at its bottom.

I have said that the greatest part of the crust of our habitable globe seems unquestionably to be stratified, and produced from detritus or fragmented materials. The other portion, though probably the least in bulk, is, generally, the most conspicuous, owing to its durability, elevation, and picturesque beauty. This kind of rock is contrasted with the former class, particularly in its negative qualities; in being, according to some geologists, altogether devoid of stratification in the general mass, and entirely free from component fragments; the whole being made up of crystalline forms, moulded upon each other, in obedience to certain chemical laws.

This crystalline rock, as the Society are well aware, abounds in the neighbourhood of Edinburgh, in Arthur's Seat, Salisbury Craigs, and in Corstorphine Hill. It is decidedly posterior to the stratified class, of which it penetrates the crevices at all angles, in the form of dykes or veins, like stucco cast in a mould; frequently also lodging between the strata in vast shapeless masses.

As the rock in question never fails to preserve this quality of universal and perfect crystallisation, I heartily concur with Dr HOPE in bestowing upon it the general name of *Crystallite*, un-

der which are comprehended all substances of this kind, including not only Whinstone and Basalt, but also Porphyry, Granite, and Sienite of every description.

The solid mass of our globe, then, in so far as it is naturally exposed to our view, or has been penetrated by the labours of the miner, would appear, (with the exception of some streams which have flowed from Vesuvius, Lipari, and other volcanoes, in which the rock possesses a glassy structure), to be comprehended under these two classes, Aggregates and Crystallites.

The whole of these rocks, of both classes, furnish, at every turn, proofs of their having undergone revolutions of the utmost magnitude ; and much ingenuity has been exerted, in endeavouring to trace these changes to some consistent and rational system. But of all the active powers of nature, one only has occurred to me as capable of affording a solution, in any degree satisfactory of the phænomena,—I mean the power of internal heat, which, in all ages, and in various countries, has made its appearance at the surface of the earth, not unfrequently from under the ocean, and which still, in our own days, gives occasional proofs of its unabated activity.

To ascertain the reality and sufficiency of this agent, and to trace the volcanic fire to its source, with tolerable probability, is, doubtless, an object of great interest and curiosity ; but it has always appeared to me, that the progress of geology was retarded by a premature anxiety to enter into such investigations.

Taking it for granted, however, as, indeed, no one can dispute, that there frequently do arise violent exertions of heat from under the bed of our ocean, Dr HUTTON held that this might furnish a rational and sufficient theory of the earth, without entering into any inquiry as to the origin of that heat ; and admitting that there are many geological facts which cannot be accounted for by such a fire as that of Vesuvius, now acting at the surface, in free communication with the air, he contended that

the case may be very different, where that same cause acts at the bottom of a deep sea, and under various modifying circumstances, by which its operation could not fail to be influenced.

This, indeed, constitutes the essence of the Huttonian Theory, which I learned principally in conversation with its illustrious author; and which, since his death, I have taken every means of submitting to a variety of chemical tests; being for ever on the watch for such natural scenes as might illustrate these principles, as well as for opportunities of making experiments, to determine whether such modifications on the action of heat were, or were not, sufficient to justify the expectations of Dr HUTTON.

It was in prosecution of these views that I formerly undertook a set of experiments, proving, I believe to the satisfaction of the scientific world, the identity of Whinstone and Lava, of which a full detail is given in your Transactions. In farther illustration of the same topic, my experiments on Carbonate of Lime were formerly undertaken, by which it was shewn, that calcareous matters, exposed to heat under pressure, might be fused; and, on cooling, would crystallise, so as in every respect to resemble marble. To these I beg leave likewise to refer the Society.

The immediate object of the paper I have now the honour of submitting to the Society—the consolidation of the strata—has been pursued in a similar spirit, and with similar views to those formerly announced. In making efforts to trace the modifications which the action of heat would undergo, when compelled to act under the influence of compression, or of other circumstances, all of which, in company, I have always been willing to distinguish by the name of *Plutonic*, (although the term was originally suggested, ironically, by one of our keenest antagonists, the late celebrated Dr KIRWAN), I was led to the particular topic of this paper, by an unexpected scene which presented itself in my own neighbourhood, in the country.

It had often been urged, and apparently with good reason, against this branch of the Huttonian Theory, that no amount of heat applied to loose sand, gravel, or shingle, would occasion the parts to consolidate into a compact stone. And as all my experience led to the same conclusion, I saw that, unless, along with heat, some flux were introduced amongst the materials, no agglutination of the particles would take place. The striking circumstance above alluded to, as occurring near Dunglass, and which will be particularly described presently, having suggested to me the idea that the salt of the ocean might possibly have been the agent in causing the requisite degree of fusion, I instituted a series of experiments, the details of which I am about to bring before the Society. By these, I conceive it will be shown, that this material, under various modifications, is fully adequate to explain the consolidation of the strata, and many other effects which we see on the surface of the Earth.

My success, from the first, was such as to promise the most satisfactory result, though it is only within the last year that I have been able to command the repetition of the experiments in a manner fit to be laid before this Society. This must be my apology to those who hear me, and to such of my friends as take an interest in these investigations, for having so long delayed the publication of a set of facts, some of which had presented themselves to my view many years ago.

Whoever, indeed, has had any experience in the prosecution of new subjects of experimental inquiry, knows that, owing to his ignorance of the requisite adjustment of the proportions of the ingredients, and of other similar arrangements, he must depend, in a great degree, upon chance for the success of his first results, and that he must often submit to spend much time and labour upon a subject, even after it has been made out to his own satisfaction, before he has acquired sufficient command over its details to answer for the result of any particular experi-

ment, so as to be able to produce it with confidence to the public.

It may be interesting, in the first place, to describe, in a general way, the geological structure of the country, in the neighbourhood of the singular scene which gave rise to these speculations.

On different occasions I have laid before this Society observations made on the rugged shore which occupies the southern entrance of our estuary the Firth of Forth, which, from being frequently washed by a very boisterous ocean, presents to view a distinct exhibition of its internal structure. The eastern part is occupied by the promontory of Fastcastle, composed entirely of the elder quality of strata, called by the Germans Grey Wacke. Further to the west it consists of cliffs formed of Sandstone, nearly in a horizontal position. These two meeting in the crag called the Siccar Point, afford the most distinct view we any where have of the peculiar relation and mutual history of these two rocks.

More inland, on the borders of Lammermuir, a set of horizontal beds occur, consisting of a loose assemblage of rounded stones, intermixed with sand and gravel, which bear every appearance of having been deposited by water, and which, as to their general history, seem to have undergone no change since the overwhelming, though transient, agitations of water, of which I have frequently had occasion to speak in this Society.

In the summer of 1812, as I was returning from visiting the granitic range which occurs in the water of Fasnet, in the hills of Lammermuir, and riding down the little valley of Aikengaw, which deeply indents this loose collection of gravel and shingle, about two miles above the village of Oldhamstocks, and at the distance of eight or ten miles from the sea, I was struck with astonishment on seeing one of these gravel banks, formed, as

above described, of perfectly loose materials, traversed vertically by a dyke, which, in its middle, consisted of whinstone, and was flanked by solid conglomerate; but this solidity abated gradually till the conglutination of the rounded masses diminishing by degrees, the state of loose shingle and gravel was entirely restored on both sides. The agglutinated mass adjacent to the dyke bore no resemblance to the result of calcareous petrifaction; scarcely ever gave effervescence with acid; and, by its gradual termination, differed from any whinstone-dyke I have seen to penetrate the strata; for, in the ordinary case, the termination of the crystallite against the adjoining aggregate through which it passes, is almost always quite abrupt.

About a hundred yards higher up the valley of Aikengaw, there occurs an agglutination similar to the last, though without any whin-dyke, and sufficiently strong to resist the elements, by which the surrounding matters had been washed away, leaving the pudding-stone, or agglutinated shingle, to stand up by itself, in a manner remarkable enough to have attracted the notice of the peasantry as something supernatural, since they have bestowed upon it the name of the Fairy's Castle.

Farther up the stream, other agglutinations occur frequently, as we could see in little narrow glens cutting through the mass; and higher still, they are so numerous as to meet and convert the whole into one unbroken mass of pudding-stone, occupying all that is exposed to view.

These very remarkable, and, to me at least, novel appearances, were the first which suggested the idea, that the consolidation not only of this class of conglomerates, but of sandstone in general, had been occasioned by the influence of some substance in a gaseous or aëriform state, driven by heat into the interstices between the loose particles of sand and gravel, where it had acted as a flux on the contiguous parts. On considering what this penetrating substance might be, and from whence it could

have come, the following circumstance presented itself to my recollection at the moment, and promised to afford some assistance to these conjectures.

A few miles lower down the valley in which the above facts were observed, at the distance of more than a mile from the sea, and between two and three hundred feet perpendicularly above it, there occurs a crag of sandstone, in which a numerous succession of strata are distinctly visible. Several of these beds have yielded much to the action of the air, and, in dry weather, exhibit a considerable white efflorescence, which has completely the taste of common salt; and so remarkable is this circumstance, that the rock has acquired, in the country, the name of Salt-Heugh.

Here, then, it immediately occurred to me, was probably the source of an abundant supply of the elastic substance or fumigator, whose action as a flux had been pointed out by the agglutinations in Aikengaw above described.

I conceived, that, if there were at the bottom of the sea a bed of sand and gravel, drenched with brine of full saturation, and that heat were applied to it from beneath according to Dr Hutton's hypothesis, the first effect would be, to drive the water from the lowest portion of the sand, and to convert the salt which remained amongst it, together with the sand, into a dry cake. During this operation, or until the cake became quite dry, the absorption of latent heat would prevent the temperature from surpassing the boiling point of brine. But no sooner was this dryness accomplished, than, I imagined, the temperature of the mass would begin to rise above that pitch; the portion of it next the fire would gradually acquire a red-heat; that then the salt, being made by the heat in part to assume an elastic form, would be sent in fumes through the dry cake just described, and thus, by partially melting the contiguous particles, produce an agglutination.

Such being my theoretical views, no time was lost in submit-

ting them to the test of experiment. Taking it for granted that a quantity of sea-salt must frequently be formed and deposited, along with sand and gravel, at the bottom of the ocean (in the manner I shall have occasion to describe at another stage of this paper), where the water has been collected by its superior specific gravity, in the form of brine, I proceeded to make the following experiments.

Dry salt was placed along with sand, sometimes in a separate layer, at the bottom of the crucible, and sometimes mixed throughout the experiment: the whole was then exposed to heat from below. I found that the salt was invariably sent in fumes through the loose mass, and by its action produced solid stone in a manner completely satisfactory, as illustrative of the facts in Aikengaw; and so as to give a good explanation of the production of sandstone in general.

These artificial stones are of various degrees of durability and hardness;—some of them do not stand exposure to the elements, and crumble when immersed in water;—some resist exposure for years;—others are so soft as not to preserve their form for any length of time;—while some bear to be dressed by the chisel; and, it may be remarked generally, that, as far as the results of my experiments have been compared with natural sandstone, the same boundless variety exists in both cases. A striking instance of this resemblance occurs in the case of the Salt-Heugh, the sandstone of which, when immersed in water, crumbles down, exactly in the same manner as those results of my experiments which taste much of salt.

The fumes of the salt, no doubt, act, in all these cases, as a flux on the siliceous matter, and thus cement the adjacent particles together. The Society are, doubtless, well aware of the power of salt fumes in glazing pottery; and the analogy, I conceive, is complete. It is the application alone that is new.

So far the results were satisfactory. But it next occurred, that it might be plausibly objected, that the presence of the superincumbent cool ocean, would interfere with the process, on

the principles of latent heat. To put this to the test, I proceeded to expose a quantity of sand, covered to the depth of several inches with common salt-water, to the heat of a furnace, and, as the liquid boiled away, replenished it from time to time by additions from the sea. Of course it gradually approached to a state of brine. But this proved a very tedious operation, requiring a continued ebullition, during three weeks without ceasing, before it became sufficiently saturated with salt by the discharge of the fresh-water; and I thought it much easier, and no less satisfactory, to employ brine from the first, formed at once by loading the water with as much salt as it could dissolve, amounting to about one-third of its weight.

The vessels employed in these early experiments, were the large black-lead crucibles used by the brass-founders. I filled the vessel, which was 18 inches high and 10 broad, nearly to the brim with brine of full saturation, the lower portion being occupied, to the depth of about 15 inches, with loose sand from the sea-shore, and thoroughly drenched with the brine. In order to have a view of the progress of the experiment, I placed an earthen-ware tube, about the size and shape of a gun-barrel, closed at bottom, and open at the top, in a vertical position, having its lower extremity immersed in the sand, and reaching to within about an inch of the bottom of the pot, while the other end rose a foot above the surface of the brine, and could be looked into without inconvenience.

After a great number of experiments, furnishing an unbounded variety of results, I at length obtained a confirmation of the main object in view. I observed that the bottom of the porcelain barrel, and of course the sand in which it rested, became red-hot, whilst the brine, which, during the experiment, had been constantly replenished from a separate vessel, continued merely in a state of ebullition: the upper portion of the sand, drenched with the liquid, remained permanently quite loose,

but the lower portion of the sand had formed itself into a solid cake.

On allowing the whole to cool, after it had been exposed to a high heat for many hours, and breaking up the mass, I was delighted to find the result, occupying the lower part of the pot, possessed of all the qualities of a perfect sandstone, as may be seen in the specimens now presented to the Society. Whenever the heat was not maintained so long, the sandstone which resulted was less perfect in its structure, tasted strongly of salt, and sometimes crumbled to sand when placed in water.

Many of these early experiments were accomplished with tolerable success. But still the result was somewhat precarious, and could not be announced with the confidence that I felt in presenting my former experiments to this Society.

The cause of this uncertainty I traced to the chemical operation of the salt, acting as a flux upon the porcelain vessels employed. This very action, I was well aware, was the main agent and cause of our success, when kept within proper bounds; but, on being allowed to pass those limits, and to act on the containing vessel as well as on the experiment, it destroyed the vessel, and converted the whole into a confused mass of slag.

After numberless unsuccessful attempts, and after returning again and again to the charge, with an interval sometimes of years, I at last met with a quality in some of the materials to me altogether unlooked for, by means of which may be obtained successful results, with scarcely any risk of failure.

I found that the action of the salt upon the substances of the crucibles of clay, did not exert itself in the same manner upon iron; but that a large vessel of cast-iron, 18 inches deep by 10 wide, and a common gun-barrel welded up at the breech, and open at the top, enabled me to work with the heat of melting gold, without injuring the vessels, and at any time to produce a perfect freestone; thus satisfying our theoretical expectations.

Similar results, in all respects, were produced by exposing pure pounded quartz to the action of the salt fumes,—and also when gravel, or any other mass of loose materials, was used instead of sand.

Having now shewn, in a satisfactory manner, that salt, whether in a dry state mixed along with loose materials, or driven in fumes through them, or applied in the state of brine, and exposed to heat, is a sufficient agent to produce a consolidation, such as we see in natural sandstones and other stratified rocks, it remains to be investigated, whether an adequate supply of this flux may be reckoned upon in nature.

It is well known that great diversity exists in the degree of saturation of the sea by salt, at different places; and BUFFON has been at much pains in collecting examples of this diversity in his geological volumes, introductory to his Natural History. It is known that, in many of the communications between sea and sea, a constant current sets one way, indicating that the evaporation from the sea, to which this stream flows, surpasses in quantity its supply of fresh-water from the rivers, rains, and springs. This is remarkably the case with the Mediterranean, into which a perpetual stream sets from the ocean, at the Gut of Gibraltar. We have reason, then, to conclude, both that the surface of the Mediterranean is lower than that of the ocean, and likewise that the quantity of salt in the former is perpetually on the increase; so that the specific gravity of the waters, and the intensity of their saturation, must be perpetually advancing to a state of brine. I am well aware, that an attempt has been made to render such a conclusion unnecessary, by the supposition of a counter-current flowing at the bottom, out of this great basin; but such suppositions are, in my opinion, altogether gratuitous.

What is here said of the Mediterranean, will apply no less to other seas, and even to the great oceans. And wherever a basin

occurs, in which a bottom of great depth is surrounded by a ridge comparatively shallow, we may expect to find the lower portion, at least, of the water in a state approaching to brine.

Without any such theoretical explanation of the manner in which a supply of salt is supposed to be formed, it may perhaps be considered sufficient for my purpose, to recal to the recollection of the Society, that there are in almost every part of the world vast districts of rock-salt, and in some countries extensive salt lakes and salt rivers; and in our own country we have many instances of brine springs, besides rock-salt in abundance.

Here then it seems to me, we are plentifully furnished with the means of accounting, in the manner experimentally shewn, for the agglutinations of such gravel as that of Aikengaw and for the strata of the Salt-Heugh, which, by an easy analogy, may be transferred to sandstone in general, and, perhaps, to stratified rocks of every description.

A member of this Society, however, well known by his scientific acuteness, alleged, first in his public lectures, and afterwards, upon my requesting an explanation of his objection, again repeated, that I was not justified in such theoretical conclusions, respecting the influence of heat at the bottom of the sea, since the neighbourhood of the cool water would necessarily counteract that influence.

In answer to this difficulty, I must beg leave to remark, that, in all my experiments above alluded to, the sand (viewed by means of the gun-barrel) was seen to become red-hot during the process of consolidation, while the superincumbent brine remained boiling above; and it was even found easy, by supplying cool brine in sufficient quantity, to maintain the temperature of the fluid permanently such, that the hand could be plunged into it at top, without injury, the sandstone below remaining all the while at a full red heat. But whenever I repeated this experiment,

with every circumstance the same, both as to duration and temperature, as in the example above detailed, but in which, instead of brine, *fresh* water was used, the result was very different. The lower part of the gun-barrel, immersed in the sand, and in which gold had melted in the brine experiment just mentioned, now remained permanently black and cold; and the whole of the sand in the pot, when removed from the furnace, fell out loose by its own weight; not the least trace of consolidation having taken place.

We may thus, I trust, presume to have added one more new and important modifying circumstance of heat, to those already advanced in support of the Huttonian doctrines; for, since it has been experimentally shewn, that heat, under the modifications produced by the presence of salt, as above described, is fully adequate to the consolidation of loose materials, exposed to its action, it may fairly be presumed, that salt has performed a part, and a very important part, in the consolidation of the strata of the globe.

I should be doing injustice to the subject, were I not to state, that, besides the views developed in the foregoing paper, and supported by actual experiment, many others have occurred to me, respecting the agency of salt under various modifications, and all bearing more or less directly upon the Huttonian Theory of the Earth. Some of these views have been submitted to the test of experiment, and the results, as far as they have yet been carried, give me great hopes of ultimate success. Others are still in the shape of mere conjecture; and none of them are yet in a state to lay before the Society in detail. A simple allusion to one or two of the most important of these views may probably be received with indulgence; and I shall be very happy if gentlemen possessed of adequate leisure, shall be induced to

follow up, by actual experiment, what I have thrown out as mere matter of speculation.

I conceive that salt, in the state of fumes, and urged by a powerful heat, possibly also modified by pressure, or perhaps combined with other substances, may have penetrated a great variety of rocks, acting as a flux on some, as in basalt, granite, &c.; agglutinating others, as in the case of sandstone, pudding-stone, &c.; softening others, as in the case of contorted strata of greywacke. In many cases, too, I conceive that these fumes may have had the power of carrying along with them various other materials, such as metals in a sublimed state, which would in this way be introduced into rents, veins, and cavities, or may even have entered into the solid mass of the rocks, which I imagine these fumes may have had power to penetrate. I have already tried some experiments in pursuit of these ideas. Salt, for instance, has been mixed with oxide of iron, reduced to fine powder, and then exposed to heat along with quartzose sand The iron, I found, was borne up along with the salt fumes. The sandstone, formed in this way, was deeply stained with iron, and other most curious appearances presented themselves.

Every one who has seen a sandstone quarry, must have noticed evident traces of iron, the rock being stained in a great variety of ways; sometimes in parallel layers,—sometimes in concentric circles, or rather in portions of concentric spheres, like the coats of an onion,—and, generally speaking, disposed in a way not accountable by deposition from water. All these appearances I would account for, by supposing the rock, either at the moment of its agglutination into sandstone, or at some subsequent period, to have been penetrated by the fumes of salt, charged with iron, also in a state of vapour.

I may mention one very curious result of my experiments with salt and iron, acting upon sand, namely, that, upon breaking up the specimen of artificial sandstone, an appearance often

presents itself of incipient crystallisation, if I may use this term; a number of large, shining, parallel faces pervade the whole mass, and, by holding the specimen at the proper angle to the light, this appearance becomes very obvious. What the nature of these crystals is, I have not investigated; but as they very much resemble what we see in different kinds of sandstone, I am of opinion that they hold out a fair expectation, of our being able to produce many of the crystalline appearances with which we are familiar in nature.

Common sea-salt, such as I have used, as is well known, is not pure muriate of soda; and, in my experiments, I have mixed various other substances with it. In Nature, we must suppose that various contaminating substances would in like manner occur, to diversify the phenomena; and, accordingly, we do find a boundless variety, in the aspect not only of sandstone, but of almost every kind of rock; and I am by no means without expectation, that, in the course of time, we shall be able to imitate in our laboratory as many of these varieties as we choose to exhibit.

I have long been engaged also in a series of experiments on the formation of *Crystallites*, the name by which, as I have before stated, every crystallised rock might, perhaps, be usefully distinguished in contradistinction to *Aggregates*, or those formed of fragments. This great object in experimental geology, I hope to accomplish by means of an instrument which I have long had in use, for the regulation of high heats, a description of which may probably soon be laid before the Society, together with some further results in support of the Huttonian Theory of the Earth.

XXIII. *Observations before and after the Superior Conjunction of Venus and the Sun, with the Mural Circle at Paramatta,* 1824. By His Excellency Sir Thomas Brisbane, K. C. B. F. R. S. Lond. & Edin.

(Read May 16. 1824.*)*

	Barometer.	Thermometer out.	Thermometer in.		Transit over the Meridian wire.	Mean of the 4 Microscopes.	REMARKS.
1824.					h. ′ ″	° ′ ″	
☉ July 17.	29.688	55.0	55.0	Aldebaran,	4 26 34 .5	184 45 36.50	—
—	-	57.8	-	Capella,	5 4 28 .5	214 21 14.22	—
—	-	-	-	Rigel,	6 48 .5	160 12 51.73	—
—	29 676	60.0	-	♀ Centre,	7 36 45 .0	191 0 8.30	—
18.	-	60.2	-	☉ Upper limb,	7 48 40 .1	189 26 31.62	AR ☉ 1st L.
☽ 18.	29.736	57.0	-	Aldebaran,	4 26 33 .0	184 45 37.35	—
—	-	59.0	-	Capella,	5 4 26 .0	214 21 13.27	—
—	-	-	-	Rigel,	6 47 .0	160 12 50.05	—
—	29.708	63.1	57.0	♀ Centre,	7 41 59 .90	190 40 52.45	—
☽ 19.	29.708	63.5	57.0	☉ Lower limb,	7 52 39 .50	189 47 31.65	☉ 1st L.
♂ 19.	29.940	46.0	55.0	Aldebaran,	4 26 30 .80	184 45 36.50	—
—	-	50.0	55.0	Rigel,	5 6 45 .0	160 12 53.75	—
—	29.960	60.0	55.4	♀ Centre,	7 47 13 .0	190 38 55.90	—
♂ 20.	29.960	60.2	55.5	☉ Lower limb,	7 56 37 .0	189 36 32.25	☉ 1st L.
☿ 20.	29.836	70.0	58.0	♀ Centre,	7 52 26 .0	190 27 20.12	—
☿ 21.	29.836	70.2	58.0	☉ Upper limb,	8 0 35 .8	188 53 35.37	☉ 1st L.
♃ 21.	29.700	65.0	61.5	♀ Centre,	7 57 37 .9	190 15 6.62	—
♃ 22.	29.700	65.0	61.5	☉ Lower limb,	8 4 33 .0	189 13 37.52	☉ 1st L.
♀ 22.	29.922	62.6	-	♀ Centre,	8 2 48 .7	190 2 8.12	—
♀ 23.	29.922	62.2	-	☉ Upper limb,	8 8 30 .0	188 29 53.05	☉ 1st L.
♄ 23.	30.110	45.3	55.8	Aldebaran,	4 26 21 .7	184 45 33.87	—
♄ 23.	30.110	49.6	55.8	Capella,	5 4 15 .5	214 21 16.47	—
—	30.110	49.6	55.8	Rigel,	5 6 35 .2	160 12 52.85	—
—	30.073	61.0	56.2	♀ Centre,	8 7 58 .3	189 48 35.25	—
♄ 24.	30.073	61.0	56.2	☉ Lower limb.	8 12 25 .5	189 49 10.65	☉ 1st L.
♂ 27.	29.960	46.0	53.3	Capella,	5 4 8 .5	214 21 0 .0	—
—	-	-	-	Rigel,	6 28 .2	160 12 54.07	—
—	29.890	58.1	54.0	♀ Centre,	8 28 31 .0	188 48 21.42	—
☿ 28.	29.890	58.1	54.0	☉ Upper limb,	8 30 21 .5	187 24 50.70	AR of ☉ following L.
☽ Aug. 1.	30.164	40.0	53.4	Aldebaran,	4 26 15 .0	184 45 38.85	—

		Barometer.	Thermometer out.	Thermometer in.		Transit over the Meridian wire. h. ′ ″	Mean of the 4 Microscopes. ° ′ ″	REMARKS.
1824.								
☽ Aug.	1.	30.164	44.8	53.0	Capella,	5 4 9.0	214 21 3.32	—
—		-	-	-	Rigel,	0 6 28.5	160 12 52.82	—
☽	2.	30.126	60.8	53.2	☉ Upper limb,	8 47 39.2	186 11 57.90	☉ 1st L.
—		-	-	-	♀ Centre,	8 53 58.3	189 20 17.15	—
—		-	-	-	Aldebaran,	9 26 14.3		—
♄	3.	29.800	65.0	54.8	☉ Lower limb,	8 53 45.2	186 28 7.95	☉ 1st L.
—		-	-	-	♀ Centre,	8 58 59.3	187 1 6.28	—
♄	4.	29.900	59.3	54.8	☉ Upper limb,	8 57 6.8	185 40 44.72	☉ 1st L.
—		-	-	-	♀ Centre,	9 5 44.6	186 41 24.07	—
♄	5.	29.924	58.8	52.	☉ Lower limb,	8 59 16.0	185 56 25.95	☉ 1st L.
—		-	-	-	♀ Centre,	9 9 1.6	186 22 5.35	—
♀	5.	30.144	37.0	-	Aldebaran,	4 26 20.0	184 45 38.87	—
♀	6.	30.102	60 0	-	☉ Upper limb,	9 3 11.2	185 8 21.10	☉ 1st L.
—		-	60.0	-	♀ Centre,	9 14 3.5	186 0 20.55	
♄	7.	30.026	-	-	☉ Lower limb,	9 7 5.7	185 23 34.12	☉ 1st L.
—		-	-	-	♀ Centre,	9 19 6.9	185 39 3.32	—
☉	8.	30.050	62.0	53.	☉ Upper limb,	9 10 59.5	184 35 1.47	☉ 1st L.
—		-	61.8	53.	♀ Centre,	9 24 7.4	185 17 20.40	—
☽	9.	30.038	61 0	-	☉ Lower limb,	9 14 52.2	184 49 31.85	☉ 1st L.
—		-	59.8	-	♀ Centre,	9 29 07.7	184 55 6.67	—
☿	11.	30.178	64.0	51.8	☉ Lower limb,	9 22 37.3	183 42 56.60	☉ 1st L.
—		-	-	-	♀ Centre,	9 39 3.9	184 9 28.35	—
♄	12.	30.202	66.0	-	☉ Upper limb,	9 26 29.0	183 25 4.58	☉ 1st L.
—		-	-	-	♀ Centre,	9 44 0.0	183 45 58.37	

REMARKS.

♄ 24th July. The Sun's limb tremulous. The observations on ♀ good. Very cloudy, with high wind, which prevented the observation being made on the 29th, 30th, and 31st July.

XXIV. *Observations on Two Comets discovered at Paramatta in 1824, by Mr Rumker and Mr Dunlop.* Communicated by his Excellency Sir Thomas Brisbane, K. C. B. F. R. S. Lond. & Edin. in a Letter to Dr Brewster, Sec. R. S. Edin. *To which are added the Elements of their Orbits, calculated by* Mr George Innes, *and* Mr James Gordon, A. M. Aberdeen.

(Read May 16. 1825.)

The two Comets which are the subject of this communication were discovered at Paramatta; the first by Mr Rumker, and the second by our countryman Mr Dunlop.

The elements of both have been calculated from the observations of Mr Rumker and Mr Dunlop, by Mr George Innes, Aberdeen, and Mr James Gordon, A. M.

COMET, *August* 1824.				
1824.	Mean Time at Paramatta.	Mean Æ of Comet.	North mean Declination.	No. of Observations.
	H.			
August 21.	8 44′ 48″,84	245° 24′ 17″,47	36° 54′ 17″,66	1
23.	7 11 13,70	244 18 58,30	37 55 14,85	11
24.	7 56 8,90	243 43 34,75	38 26 20,12	4
26.	8 1 0,39	242 38 44,46	39 25 24,65	6
27.	7 51 32,63	242 7 31,00	39 45 33,60	7
28.	7 26 36,32	241 37 14,30	40 22 49,24	4
29.	7 41 49,99	241 7 40,70	40 48 10,80	3
31.	7 24 19,26	240 11 32,85	41 42 20,01	5

1824,

Aug. 21. Comet north, preceding anonymous star of the 7th magnitude, Æ of the star = 246° 44′ 2″,47; declination 36° 49′ 22″,18; diff. in Æ 1° 19′ 45″,0; diff. in decl. 0° 4′ 55″,48.

23. Comet north, preceding 25 Hercules, FLAMSTEAD's Catalogue, diff. in Æ 29′ 9′,60; in decl. = 7′ 40″,35.

24. Comet south, following anonymous star of 7th magnitude, Æ of star = 241° 44′ 3″,30; declin. of star 38° 30′ 33″,32; diff. in Æ = 1° 59′ 31″,45; diff. in decl. = 4′ 13″,20.

26. Comet south, following 31 Hercules, BODE's Catalogue, diff. in Æ = 1° 9′ 37″;70; diff. in decl. = 4′ 40″,35.

27. Comet south, preceding 49 Hercules, BODE's Catalogue, diff. in Æ = 1° 21′ 16″,30; diff. in decl. = 13′ 31″,70.

28. Comet north, preceding 49 Hercules, BODE's Catalogue, diff. in Æ = 1° 51′ 33″,00; diff. in decl. = 14′ 44″,54.

29. Comet north, following 13 Hercules, BODE's Catalogue, diff. in Æ = 2° 8′ 27″,0; diff. in decl. = 17′ 19″,20.

31. Comet north, preceding anonymous star of the 7th magnitude, Æ of the star = 240° 51′ 25″,25; declination of star = 41° 33′ 28″,47; diff. in Æ = 39′ 52″,40; diff. in decl. = 8′ 51″,54.

Middle of Observations in Sidereal Time.

	D.		H.	′	″
Aug.	21.	at	18	43	35,70
	23.	...	17	17	38,30
	24.	...	18	6	37,43
	26.	...	18	19	22,83
	27.	...	18	13	50,05
	28.	..	17	52	46,20
	29.	...	18	11	56,00
	31.	...	18	2	18,50

Elements of Comet.

1824.	MR INNES.	MR GORDON.	
	D. H. ′ ″	D. H. ′ ″	
Time of the Perihelion passage,	Sept. 29. 7 23 26	Sept. 29. 7 25 10	M. Time at Paramatta.
Longitude of the Perihelion, ...	4° 23 12	4° 22 11	
Place of the Ascending Node,...	279 17 56	279 19 13	
Inclination, ...	54 22 14	54 22 22	
Perihelion distance, ...	1·048553	1·048739	
Motion direct.			

This Comet is evidently the same as that which was first discovered in Europe on the 23d of July by M. SCHEITHAMMER of Chemnitz, and the elements of which, as computed by CAPOCCI, CARLINI, and ENCKE, have been published in the *Edinburgh Journal of Science*, vol. ii. p. 171, 172., and vol. iii., where the latest elements are given, upon the hypothesis of a hyperbolic orbit, which has been found by M. ENCKE to represent the observations better than a parabolic one.—D. B.

COMET of July and August 1824.

1824.	Mean Time at Paramatta.	Mean Æ of Comet.	North mean Declination.	No. of Observations.
	H. ′ ″	° ′ ″	° ′ ″	
July 28.	6 38 57,11	165 50 57,70	16 1 24,0	12
29.	6 49 24,29	166 43 15,30	16 41 4,72	8
31.	6 53 35,24	168 18 6,20	17 53 37,25	12
Aug. 1.	6 46 1,41	169 1 58,80	*18 27 14,56	10
3.	7 0 33,34	170 24 9,80	19 30 25,74	14
4.	6 50 17,73	171 0 52,85	19 59 40,65	9
6.	6 54 44,46	172 9 20,30	20 55 15,23	6
8.	6 40 28,72	173 11 34,6	21 44 41,35	1
11.	6 50 56,35	174 40 29,0	22 54 5,20	1

1824,

July 28. Comet south, preceding θ Leonis, diff. in Æ = 24′ 31″,30; diff. in declin. = 21′ 57″,61.

29. Comet north, following θ Leonis, diff. in Æ = 27′ 46″,30; diff. in declin. = 17′ 43″,12.

31. Comet south, preceding anonymous star of the 7th magnitude, Æ of the star = 168° 35′ 37″,50, and the declination 18° 6′ 6″,30 North; diff. in Æ = 17′ 31″,30; diff. in declination = 12′ 29″,05.

Aug. 1. Comet north, following another anonymous star of 7th magnitude, Æ of the star = 168° 47′ 40″,80; declination = 18° 18′ * 03″,37; diff. in Æ = 14′ 18″,0; diff. in decl. = 9′ 11″,19.

3. Comet north, following 86 Leonis, diff. in Æ = 4′ 0″,80; diff. in decl. = 8′ 11″,54.

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* In the original it is inserted 18° 80′ 03″,37. It probably should have been 18° 18′ 3″,37. This gives 18° 27′ 14″,56 for the north declination, as in the Table.

1824,

Aug. 4. Comet north, preceding a small anonymous star, 8th magnitude, Æ of small star = 171° 1′ 9″,0; declination = 19° 46′ 59″,20; diff. in Æ = 0′ 16″,15; diff. in declin. = 12′ 41″,45.

6. Comet south, following 417 Leonis, Bode's Catalogue, diff. in Æ = 40′ 46″,20; diff. in declin. = 29′ 44″,17.

8. Comet north, following 417 Leonis, diff. in Æ = 1° 43′ 0″,5; declination 19′ 41″,95.

11. Comet south, following 445 Leonis, Bode's Catalogue, diff. in Æ = 1° 27′ 0″; diff. in declin. = 17′ 9″,80.

Middle of Observations in Sidereal Time.

	D.		H.	′	″
July	28.	at	15	2	46,06
	29.	...	15	17	11,46
	31.	...	15	29	16,17
Aug.	1.	...	15	25	37,91
	3.	...	15	48	5,14
	4.	...	15	45	40,95
	6.	...	15	54	4,91
	8.	...	15	47	40,0
	11.	...	16	9	59,0

Elements of Comet.

1824.	Mr Innes.	Mr Gordon.	
	D. H. ′ ″	D. H. ′ ″	
Time of the perihelion passage,	July 10. 10 17 30	July 10. 10° 17 41	M. Time at Paramatta.
Longitude of the Perihelion, ...	259° 45 32	259 45 31	
Place of the Ascending Node, ...	330 29 8	330 29 8	
Inclination, ...	57 0 36	57 0 36	
Perihelion distance, ...	0·5956114	0·5956147	
Motion retrograde.			

This comet, which was not seen in Europe, is stated by Dr Olbers to have been discovered by Mr Rumker, and to have been observed by him from the 15th July to the 6th August. The following are the elements given of it by Dr Olbers, as computed by Mr Rumker:

		H.	′	″
Time of Perihelion passage, Mean Time at Paris,	July 11.	12	26	27
Longitude of the Perihelion,..		260°	16	32
Longitude of the Ascending Node, ...		234	19	9
Angle between the Perihelion and the Node, ...		334	2	37
Inclination of Orbit, ...		54	34	19
Perihelion distance,..		0·591263		

Motion retrograde.

The difference between these elements and those obtained by Mr GORDON and Mr INNES is very considerable. Dr OLBERS remarks, that the observations might have been reduced with greater accuracy; and he adds, that we may perhaps expect additional ones from Sir THOMAS BRISBANE or Mr DUNLOP.—D. B.

XXV. *On the Construction of Meteorological Instruments, so as exactly to determine their Indications during Absence, at any given instant, or at successive intervals of Time.* By HENRY HOME BLACKADDER, Esq. Surgeon, MED. STAFF H. P.

(Read May 2. 1825.)

It is universally admitted, that, in the present advanced stage of Meteorological Science, nothing would be more desirable than complete and accurate registers of the indications of Meteorological Instruments,—more especially of the Thermometer, Barometer, and Hygrometer, such registers being cotemporaneously kept at numerous places, and at various elevations on the earth's surface. Many are the obstacles, however, which have been found opposed to such an acquisition.

There are probably but few of those at all conversant with meteorological pursuits, who have not been induced, at one time or other, to commence keeping a register. But the necessary regular inspection of instruments at certain fixed hours of the day, and for many months or years in succession, has, in most instances, been found to become so irksome, so liable to unavoidable interruptions, and so apt to interfere with other equally important avocations, that few, indeed, have been able to persevere for such a length of time as was necessary to arrive at any very decided results. As to most of those registers to be found in circulation, it is well understood, that little or no dependence can be placed on their accuracy. But, even in those instances in which accuracy may be expected, if we advert to the great di-

versity of opinion as to the hours at which the indications of instruments ought to be noted; and, consequently, the want of correspondence between registers kept by different individuals, the difficulty, or rather the utter impossibility, of deriving any positive results from a comparison of these registers, must be abundantly evident. The invention of a thermometer that registers accurately the highest and lowest temperatures that may occur during a given period, was doubtless a great acquisition, and such an instrument has been found of much use in many highly interesting investigations. If, indeed, it were ascertained that the mean of the highest and lowest temperatures of the day was exactly equal, or bore a known and uniform relation to the mean temperature of the whole day, then would such an instrument be of great utility in determining the general and important problem of mean temperature. This, however, has not yet been ascertained, nor can such a problem be solved without much labour, and an infinity of future observations. The result of some recent observations would certainly lead to the conclusion, that the mean of the extremes is not the mean of the daily temperature in such countries as Britain, more especially at certain seasons of the year, and in certain years, or series of years, less than in others.

It has been acknowledged, that the only way by which the mean daily temperature can be determined with accuracy, is by taking the mean of observations made frequently, and at least once every hour, during the whole period of the day and night; and nothing but the hitherto extreme difficulty of putting such a method into execution, has prevented its adoption. Some few individuals acting in concert, and by turns, have persevered several weeks in registering the hourly indications of the thermometer. But, little is to be expected from such exuberant zeal; and, even those most likely to engage in such undertakings, are

not such as would inspire that confidence in regard to accuracy, that is so indispensably requisite in all such undertakings.

It has been considered possible, however, to procure an accurate hourly register of the thermometer, by engaging a number of individuals to act by turns, as registers, and for a pecuniary compensation. But even this method (evincing, as it does, a true devotion to science), there is reason to fear will prove but too defective. It must be attended with great expence, and hence can but rarely, and only in particular circumstances, be put in practice. On the other hand, the accuracy, and even the good faith of those who might agree to sell their time for such a purpose, will by many be considered as questionable; so that in the end, and after no inconsiderable trouble and expence, a lengthy register, of which, it is said, it may or may not be accurate, is perhaps the only reward. That this would be the probable result in many instances, is the opinion of those who have had an opportunity of forming a judgment in cases similar to the one in question: and if I may be permitted to express my own opinion, derived also from some little experience, I would say, that, in general, on such occasions, some ingenious contrivance, of the nature of a tell-tale, would be about as necessary to insure accuracy, as a well-constructed thermometer.

It would be an ungracious task thus to enumerate so many difficulties and deficiencies, had I no method to propose by which these might be, in some measure at least, mitigated and supplied. But it was to this end alone that the preceding remarks were directed. For many years my intention had been directed to meteorological pursuits, having always in view, as may be supposed, their connection with, or application to, physiology and pathology,—a connection which has hitherto proved so lamentably fruitless. I had, consequently, often with others had occasion to experience and regret the obstacles and difficulties to which I have adverted; and I had often thought of a me-

thod by which it seemed probable that these might be overcome or obviated. It was not, however, till within the last eighteen months that circumstances permitted my bringing that method to the test of experience.

For upwards of a year I have been in the daily habit of using a thermometer, the indications of which may be registered at any given instant, during absence, and so as to render the tenth of a degree on Fahrenheit's scale readily distinguishable. Suppose, then, a gentleman occupied in keeping a register of the variations of atmospheric temperature,—taking two observations in the course of the day,—say 10 A. M, and 4 P. M., which, if not the most proper, are about the most convenient hours. To carry on his register, he must be at home and disengaged every day, and exactly at these hours, which must be exceedingly irksome, if at all possible, for any length of time; or he must every now and then trust to others, little skilled perhaps, or in no way interested in such pursuits,—circumstances sufficient to render the whole register defective. By the method which I have to propose, this obstacle is completely obviated. For the observer may be from home many hours before and after the hour fixed upon for noting his register; and, on his return, he shall find the temperature of that hour exactly registered by the instrument.

If we can thus succeed in effecting a register of one observation at a given instant during absence, it is obvious that we may so adapt matters as to have a register at successive intervals,—say at the distance of one hour, half an hour, or two hours, between each; and I shall presently have an opportunity of demonstrating, that, with very little trouble, we can thus ascertain the exact temperature every hour during the whole course of the day and night.

If, again, a gentleman wished to carry on a set of experiments at two or more places at the same time, such as by the sea-shore, and at some inland situation, or at the foot and at

the summit of a mountain, or tower, he would thus have his operations greatly facilitated, and insured of accuracy.

The principle of this contrivance applies either to the spirit or mercurial thermometer, and consists in keeping a small index suspended at, or in contact with, the extremity of the fluid in the stem of the instrument; so that the former shall accompany the latter in all its movements, until the instant arrive when we wish to determine the existing temperature. At this instant the index is so acted upon as to remain fixed to its place, while the fluid either passes beyond, or retires below it.

When a spirit thermometer is used, the bore of the tube, and the weight and form of the index, require attention; but the adjustment is not difficult. As to the spirit, there is a certain strength which seems to answer best, and it must be colourless, of some age, and carefully and repeatedly filtered. The colouring matter usually added to spirit-thermometers, is in this instance of no use, and would be injurious. For, after a time, the colouring matter is partially deposited, and particles of this getting into the stem of the instrument, would interrupt the movements of the index. It is for the same reason that old spirit and frequent filtration are requisite; for if the spirit is new, and if not frequently and carefully filtered, small whitish flocculi, or minute fibres, may be seen suspended in the fluid, from which interruption to the index is liable to take place. I had, on one occasion, much trouble in adjusting an index, and, at length, discovered, that the whole had arisen from a very minute particle of colourless glass, which had by some accident got into the stem of the instrument. With proper care and attention, however, nothing is more simple than the construction of a good and perfectly accurate spirit-thermometer, for meteorological purposes Nothing, at the same time, is more rarely to be met with; for

such instruments as are usually met with, are exceedingly inaccurate, and altogether unfit for scientific purposes.

When a thermometer has been constructed in the way I have described, all that is necessary to keep the index constantly and exactly at the summit of the fluid, whatever change of temperature may take place, is to invert the instrument, and retain it either in a perpendicular or somewhat inclined position; the attraction of the fluid to the index being quite sufficient for the suspension of the latter, and for overcoming its friction on the sides of the tube. When, however, the instrument is placed in a horizontal position, the index no longer accompanies the fluid in all its motions; for if the temperature rises, the fluid passes the index as if no such body were present; and if the temperature is diminished, the index is dragged along by the fluid. Upon this latter property, the *Psychrometer*, or instrument for registering the lowest temperature, was constructed. If, then, we take such a thermometer as I have described, and suspend it vertically, and in an inverted position, *on a moveable axis*, it is obvious, that, by connecting with it a time-piece, we can have it placed in a horizontal position at any given instant. And if we also make provision, that the instant the instrument comes to its horizontal position, its bulb is exposed to a higher temperature than that of the air, it is evident that the index will point out the exact temperature of the air at the time the instrument was changed from its vertical position, and that it will continue to do so as long as the instrument retains its new position, and has its bulb kept at a higher temperature than that of the air.

This, however, will be better understood from the inspection of a model, than from description. The instrument is rudely constructed; but in other respects it is perfectly accurate, having been in daily use for the last 15 months; and, during the coldest and most stormy, as well as during the hottest weather, it has always given perfect satisfaction.

After thus fully detailing the principle, it will not be necessary to enter very particularly into the subsidiary details. The particular form or construction of the instrument being immaterial; it may be modified in many ways, agreeable to the particular views of each individual.

The means by which the bulb of the instrument is kept at a higher temperature than that of the air, is the aqueous vapour originating from the flame of a lamp; and, in the coldest stormy weather, the flame does not require to be larger than that produced by, at most, two small cotton threads immersed in oil. When gas is at command, that is doubtless the most convenient combustible, as a minute flame can be kept up almost interminably, and without requiring any attention. But it is not difficult to construct a lamp for burning oil, so as to answer every desirable purpose, and not requiring inspection or adjustment once in twenty-four or more hours.

I may here notice, that a lamp of very simple construction, can be made to burn oil *without any wick*, giving out an intensely white light, and furnishing so regular a supply of heat, that a thermometer *on the outside of a window*, with which it may be connected, shall not vary half a degree in the course of many hours.

When a mercurial thermometer is used, the difference is, that, in this case, the instrument is not placed in an inverted position; and, when it is brought into a horizontal position, the bulb, instead of being kept at a higher, must be kept at a lower temperature than that of the air. This can readily be effected, by providing the means for supporting a continual evaporation from the surface of the bulb. When the instrument receives its horizontal position, the bulb is made to come into contact with a soft hair-pencil, of a hollow circular form, through which distils *guttatim*, and slowly, from a reservoir, some evaporating fluid. On some occasions, as in a very humid state of the atmosphere,

ether may be requisite; but, on most occasions, rain-water is sufficient; the use, however, of common ardent spirits for such a purpose, is attended with but a trifling expence, and may be found convenient.

Having thus shown how the temperature of the air and other bodies may be determined, during absence, and at any given instant, it may readily be conceived, how it may, in like manner, be determined at successive intervals of time, by multiplication, and fitting arrangement of the same means. Thus, omitting various less complete combinations, seven thermometers of the before-mentioned construction, connected by a very simple piece of mechanism, will enable us to determine the exact temperature every hour during the whole course of the day and night, and that with very little trouble. For, to obtain this, it is necessary to inspect the instrument only three times in the course of the day, or during that period not usually appropriated to sleep; for example, at 7 A. M., 4 P. M., and 11 P. M.

It must be obvious, however, that there is nothing to prevent the number of inspections being reduced to one in the twenty-four hours, if there was a sufficient reason or motive for doing so.

The instrument for registering the hourly atmospheric temperature, and which is now exhibited to the Society, is intended to be fixed on the outside of a window. It must be evident, however, that the dimensions are much greater than are at all necessary; for if constructed by an expert workman, the mechanism connected with the thermometers might be reduced to about the size of a common musical snuff-box.

Having thus endeavoured to describe a method of registering the indications of the thermometer at any given instant, and

at successive intervals of time, the application of the same principle to the registering of the hygrometer will not require much illustration: For, if it be admitted that the atmizomic hygrometer (that is, a hygrometer constructed on the Huttonian principle), may be depended upon, all that is requisite to procure an accurate register is, to attach two thermometers to one slip of metal, on which is engraved a scale for each, and to keep one of the bulbs moist with water. When at any instant the instrument thus constructed is brought into the horizontal position, the index in the one tube will indicate the temperature of the air, and that in the other the temperature produced by evaporation. Nothing is more simple than this modification of the registering apparatus, for nothing can be more easily effected than keeping one of the bulbs moist with water, and in this only does it differ from that fitted to register the atmospheric temperature alone. It therefore appears, that, with very little trouble, we can ascertain the hourly variation of the temperature and humidity of the atmosphere; and we have the means of greatly facilitating other thermometric and hygrometric investigations.

Hitherto (I speak to the extent of my own information) there has been no method devised for registering even the extremes of the barometric changes, which does not infer a very considerable increase of mechanical friction; and which, consequently, does not include a degree of inaccuracy no way consistent with the present advanced state of meteorological science. For it is admitted, that, at the present day, a variation in the elevation of the mercurial column to the five hundredth part of an inch, must be attended to by those who aim at scientific accuracy.

The principle of the method for registering the indications of the barometer which I was led to adopt, consists in cutting off, at a given instant, all communication between the atmos-

phere and the mercury of the barometer, than which certainly, nothing can be more simple. If, at a given instant, the communication between the air and the mercury be cut off, the height of the mercurial column must remain unaltered by any change in the pressure of the atmosphere, until such time as the communication is restored. And nothing is more simple than the means by which this interruption of communication can be effected, during absence, and at any given instant.

An accident, by which my instrument was broken, has put it out of my power to exhibit a barometer constructed on this principle. The principle, however, is so self-evident, and the method of putting it into execution so simple, that little or no illustration seems requisite. I have therefore made a section of the cistern *, which will be quite sufficient for any explanation that may be considered necessary. The cistern is made of iron, and is about two inches in diameter—the depth of mercury in the cistern, and the distance between the surface of the mercury and the top of the cistern must be as small as the correct operation of the instrument will admit of. To the air-orifice is attached an air-tight stop-cock, and by means of a lever, or other piece of mechanism, connected with a timepiece, the stop-cock may be shut at any given instant. In this way he can ascertain the exact height of the mercurial column during absence, and at any hour or minute that may be fixed upon.

As we found to be easily effected in the case of the Thermometer and hygrometer, so also in this, by combining several such instruments in one piece of mechanism, we can have the exact height of the barometer every hour in the course of the day and night. Thus †, seven barometers, arranged at equal distances around a hollow column of wood, four inches in diameter, and

* See Fig. 1. Plate XIII. † See Fig. 2. Plate XIII.

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Fig. 4.

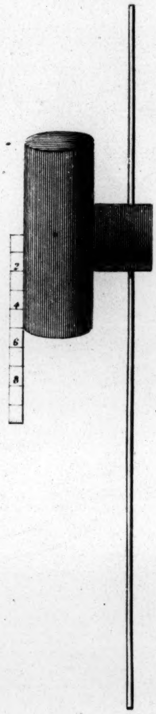


Fig. 1.

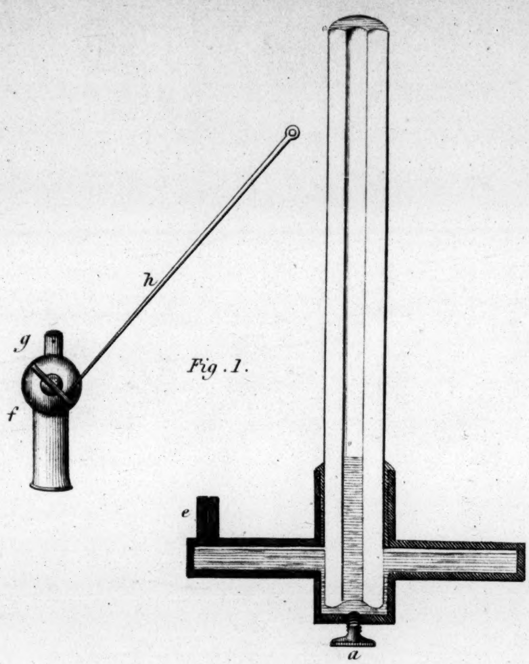


Fig. 2.

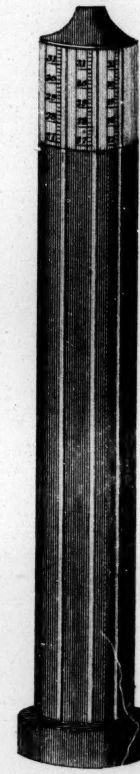
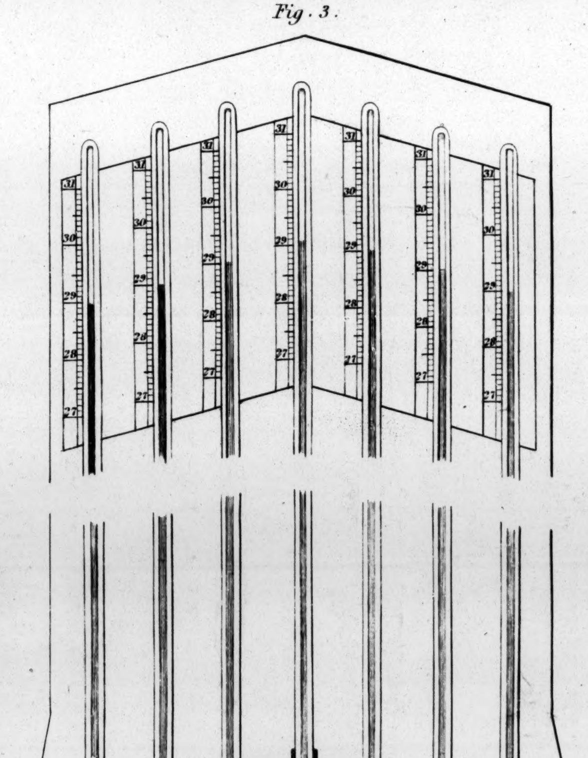


Fig. 3.



W. H. Lizars sc.

about three feet in height, having a projection at the base, in the form of a pedestal, would form not only an elegant but a very complete and highly useful barometrical apparatus. The column being hollow not only lessens the weight, but admits of the timepiece and connecting mechanism being entirely concealed within it

The barometer, however, may also be arranged on a flat surface * without producing any thing of an unwieldy appearance, and the adaptation of the mechanism for shutting one of the stop-cocks each hour in succession, is not thereby rendered more difficult.

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* See Fig. 3. Plate XIII.

EXPLANATION OF PLATE XIII.

Fig. 1. Represents a section of an iron cistern for a barometer.

d. An orifice for the introduction of the mercury, afterwards shut up by means of a screw.

e. The air-duct, having a screw formed on its outer surface.

f. An air-tight stop-cock, having a female screw, by which it is attached to the air-duct.

g. A small orifice in the side of the stop-cock, to serve as a passage for the air, and so as to exclude dust.

h. A lever connected with a timepiece, by means of which the stop-cock was shut, and the communication of the air with the mercury cut off at any given instant.

Fig. 2. Represents an instrument for determining the height of the barometer each hour in succession, by three observations in the course of the day.

It consists of a hollow column of wood about 4½ inches in diameter, and 34 inches in height; the base being about 2 inches in height, and 6½ in diameter.

It will be found convenient to have the column divided longitudinally into two equal parts, and united by means of hinges.

At equal distances, on the circumference of the column, are arranged seven barometers; their cisterns being inclosed in the base, and so placed that their stop-cocks shall form a circle in the interior. By means of a horizontal wheel nearly on a level with the stop-cocks, and which, from its connection with a time-piece, revolves once in seven hours. One of the stop-cocks are shut each hour in succession. The adaptation of the mechanism is so free of all complexity, as to render a more particular description unnecessary.

Fig. 3. Represents the seven barometers arranged on a flat surface.

a. This circle points out the situation of a vertical wheel which revolves once in seven hours. Levers are connected with each of the stop-cocks; and their central extremities, being placed at equal distances, and forming a semicircle around the circumference of the upper half of the wheel, one of the stop-cocks are shut each hour in succession.

A spiral or other spring for turning the stop-cocks, and operated upon by the levers by means of a catch, both simplifies the operation, and secures accuracy to any given instant.

XXVI. *An Examination of Dr* PARR's *Observations on the Etymology of the word Sublimis.* By GEORGE DUNBAR, A. M. F. R. S. E. Professor of Greek in the University of Edinburgh.

(Read January 9. 1826.)

IN the course of some inquiries into the affinity and structure of the Greek and Latin languages, I was led to analyse the superlative degree of both *, and to trace, as I thought, some connection between it and the word *Sublimis*. While engaged in the investigation, I was naturally led to examine the common theories respecting the etymology of this remarkable word, and, in particular, the origin assigned to it by the late Dr PARR, and to weigh, with more attention than I had previously done, the arguments and proofs he had advanced in support of his opinions. All that I knew of them, till lately, was by verbal report, as I had not seen the abridged statement of them in an Appendix to the Notes of the 2d edition of Mr STEWART's Essays.

Of the immense erudition of the late Dr PARR no one can have a more profound admiration than myself. If I might, however, be allowed to express my opinion of his merits as a scholar, I would say that the extent of his memory was prodigious; that his knowledge of classical literature was, perhaps, beyond that of any man of his day; but that his judgment was sometimes warped by prejudices and opinions, which he adopted with enthusiasm; and upon which he brought the boundless stores of his

* On this subject I shall probably, in a short time, submit to the Society a few observations.

knowledge to bear in so many shapes, and in such a variety of ways, as to confound and appal his opponents.

To Dr Parr's notions respecting the origin of the word Sublimis, Mr Stewart has given his assent more hastily, and in more unqualified terms, than might have been expected from his habitual caution, and the low estimate he had previously formed of the common derivation of the word. "As for the etymology of Sublime" *(Sublimis)*, says he, "I leave it willingly to the conjectures of lexicographers. The common one, which we meet with in our Latin dictionaries (q. Supra limum) is altogether unworthy of notice." This note, in the 1st edition of the Essays, called forth, it is understood, a long and learned dissertation from Dr Parr, the substance only of which Mr Stewart has given in the Appendix above alluded to. In the 2d edition, he says, "I have allowed the foregoing sentence to remain as it stood in the former edition of this book, although I have since been satisfied, by some observations kindly sent me by my very learned, philosophical, and reverend friend Dr Parr, that the opinion which I have here pronounced with so much confidence is unsound. The mortification I feel in making this acknowledgment is to me more than compensated, by the opportunity afforded me of gratifying my readers with a short extract from his animadversions," &c. When two men of such celebrity, the one generally reckoned the greatest classical scholar of his age, the other the most distinguished metaphysician of this or any other country, concur in the same opinion respecting the etymology of a word, which has been so long and so often disputed, it may seem to be presumption of no ordinary kind, to attempt to call in question their decisions. I derive, however, no small degree of encouragement, from finding that I am supported in my opinions by one of the most acute scholars of the present day, Dr Hunter, the Professor of Humanity in the University of St Andrew's, who, in some notes, lately put into my hands by a common friend, to

whom I communicated my objections to Dr Parr's theory, has pointed out a few of its fundamental defects. With some of his statements, however, I find I cannot agree; but this is not the place or the time to discuss them; my present business is with Dr Parr's theory. The general remarks of that profound scholar (contained in Article I. of the Appendix to the 2d edition of Stewart's Essays) on the power of custom and habit to communicate grandeur and dignity to expressions, which, in their primary acceptation, suggest low, and sometimes disagreeable ideas, but which, when compounded with other words, and applied metaphorically, convey more elevated notions, I shall pass over, as, however true they may be in particular instances, they will not hold in every other. His arguments and examples in support of the derivation of Sublimis from *Supra-limum*, I shall examine with as much care and attention as I can.

"In the formation (says the Doctor) of *Sublimis*, the process of the mind seems to me to be this: *Limus* has the property of "obstructing." That to which the word *Sublimis* is applied, is "raised above the obstructing cause." It can soar,—it does soar,—and thus the notion of "soaring indefinitely" is familiarised to the mind. The origin of the word, and its literal signification, did not present themselves to the speaker or hearer."—It has too often happened in etymological speculations, that persons particularly conversant with them, are very apt to be led astray by a similarity in the sound of words, and to task their ingenuity to the utmost to discover some kind of association between them.

If we inquire into the meaning of the word *limus* in the best Latin authors, we shall, I believe, scarcely find an instance where the property of "obstructing" is attributed to it. It sometimes denotes "tenacity," as in the following passage from Virgil—*Georg.* iv. v. 45.

> "Tu tamen e *levi* rimosa cubilia *limo*
> Ungue fovens circum"——

In Horace, *Sat.* i. v. 59. it conveys the idea of "*muddiness.*"

> "At qui tantulo eget, quanto est opus, is neque *limo*
> Turbatam haurit aquam."——

But suppose it even did convey the idea of "obstructing," should we thence infer that *sublimis* was employed to denote "raised" above the "obstructing cause;" and hence, as a consequence, the notion of "soaring indefinitely?" In tracing the gradual and successive transitions in the meaning of words, every link in the chain of the different relations should be distinctly traced, otherwise, if we supply them by the mere effort of imagination, we may rest assured there is something wrong in the process. For every effect there must be an adequate cause; and the mind must have some object in its view to carry it from the "obstructing clay" to the regions above. Has Dr Parr mentioned a single instance of any object, remarkable by its figure, magnitude, or any extraordinary property, emerging from *tenacious clay,* soaring to the regions of infinity, and drawing the astonished gaze of the world to witness its *sublime* ascent? or, have any of the writers, who have attempted to explain the nature of the sublime, produced an example that could in any shape lend the least colour to his theory? Not one. Nature exhibits nothing of the kind; and as the application of the terms of language is chiefly borrowed from the appearance of natural objects, we may thence conclude, that the Doctor's theory is fanciful and unsatisfactory.

Dr Parr's main argument, however, rests upon the meaning he supposes the preposition *sub* conveys of *elevation,* when compounded with another word; for, "when standing alone," he allows, it never has the sense of "*up.*" "An objector," he remarks," might start up and say, How is it that, in the Latin language, *sub* means "under," and "above" or "up?" I admit the fact (says he), but contend that the same letters, with the same

sound, are of different extraction, and so different as to be adapted even to contrary significations. Let it be remarked that I am going to speak of *sub*, when compounded with a verb, to express "*elevation*."

Before entering upon an examination of the examples Dr Parr has produced in support of his theory, I shall inquire whether the derivation he has given of the preposition *sub*, when compounded with verbs denoting "elevation," be correct. The Latin preposition *sub* has always been considered as formed immediately from the Greek preposition ὑπὸ, and *super* from ὑπὲρ; the Latin, as it is well known, substituting in several instances an *s* for the Greek spiritus asper. Dr Parr, however, says, that when *sub* signifies "elevation," it came from ὑπὲρ, and that ὑπὲρ, like ὑπὸ, lost the closing letters, and that *p* was changed into *b*. He adds, "I never saw this stated in any book, directly or indirectly, but no conjecture was ever more clear, or more satisfactory to my mind; and *it solves all difficulties*." Notwithstanding the high authority of the Reverend Doctor, I suspect few Latin scholars would be inclined either to derive the preposition *sub* from ὑπὲρ, or to allow that *sub*, in composition with any verb, was ever used by any Latin author, with the force and meaning of *super*. To me it appears, that he has entirely mistaken the precise meaning of *sub* in composition with some particular verbs: for in its general acceptation it cannot, by any shew of reasoning, be confounded with *super*. I state it as my decided opinion, and I am sure to be supported by every scholar who knows any thing of the subject, that, whenever *sub* occurs in composition, even when it may appear to denote "elevation," it is derived from ὑπὸ, and not from ὑπὲρ. The Greek preposition ὑπὲρ stands in the relation to ὑπὸ as *comparative* to *positive*, as it is unquestionably the fragment of the comparative ὕπερος, by a syncope for ὑπερ-ώτερος; as ὕπατος, the superlative, is for ὑπερ-ώατος. Let it be observed, that ὑπὸ denotes *under*, but always in relation to a *higher* object; and hence, where-

ever it is applied in the Greek language, either in a compound or simple state, or its derivative *sub* in Latin, it expresses the *relations* of a *lower* and a *higher* object. This I shall endeavour to prove by examples. In Homer's description of the dove, struck in mid-air by Meriones, the preposition ὑπὸ occurs in two forms. *Il.* Ψ. 874.

> Ὕψι δ' ὑπαὶ νεφέων εἶδε τρήρωνα πέλειαν,
> Τὴν ῥ' ὅγε δινεύουσαν ὑπὸ πτέρυγος βάλε μέσσην.

In the first place, the adverb ὕψι has no reference to a higher object, but only denotes the *elevation* of something *above* the position of another. It is evidently the dative by abbreviation of ὕψος. In the second place, ὑπαὶ, which I take to be the old dative feminine of the adjective ὑπὸς, is synonymous with ὑπὸ, and both point out the relative situation of an object to another *above* it. The dove was seen circling in air. Its situation might have been pointed out in relation to Meriones, who was standing on the ground; and, if the Poet had resolved so to describe it, he would have employed the preposition ὑπὲρ, not ὑπὸ. He could not, however, by such a limited relation, convey an adequate idea of the *height* of the dove *above* the spot where Meriones was standing; he, therefore, employed a preposition which expressed a kind of double relation, that of a *lower* to a *higher* object, and, by inference, the relation of space between Meriones and the dove, ascertained by its height *under* the clouds. These remarks will also apply to the expression ὑπὸ πτέρυγος βάλε μέσσην. The wound was inflicted *under* the wing; *i. e.* the wing was *higher* than the wound; or, *vice versa*, the wound was in a part *low* in comparison to the situation of the wing. As far as my experience goes, I know of no example in the Greek language where ὑπὸ, either in its simple or compound state, has any other signification than *under*, relatively to a *higher* object; and even this idea may be traced in some words

whose general acceptation is very remote from the literal meaning of their component parts.

But Dr Parr, aware that *sub*, derived from ὑπὸ, would not bend to his theory, by one of those stretches of imagination to which etymologists usually have recourse when they are grievously puzzled, derives it immediately from ὑπὲρ. I apprehend, however, that, in no instance, is the Greek preposition ὑπὲρ expressed by any other Latin preposition than *super* in compound words; and the Roman writers never confounded *super* and *sub* together. The learned Doctor, as I shall immediately shew, has not attended to the relations which the preposition *sub* in composition frequently denotes. Let us examine some of his examples, and try whether the explanation given above of the Greek preposition ὑπὸ, will not also apply to its Latin representative *sub*:

"Quantum vere novo viridis se subjicit alnus."—Virg. *Ecl.* x. v. 74.

Servius, "Subjicit, vel *sursum jacit*, vel subter jacit." Suppose we were to substitute *super*jacit for *sub*jicit, what would be the meaning of the term? It would be asked, "throws itself *over* or *above*" what? Could this be the meaning of the poet when he employed the compound verb *subjicit?* Assuredly not. Is it not evident, that he meant to express "the progress in growth which the alder makes at the commencement of spring, compared with its former *low* state?" *It shoots up, i. e.* from a *low* to a *higher* state.

"Infrænant alii currus, aut corpora saltu
Subjiciunt in equos."——Virg. *Æn.* xii. v. 288.

Servius, "*Subjiciunt in equos*, super equos jaciunt; sed proprie non est locutus, magisque contrarie, nam *subjicere* est aliquid *subter jacere*." Dr Parr thinks, that although the scholiast was puzzled with the word *subjiciunt*, "he is confident in his ability

to solve the difficulty, even to the satisfaction of Mr STEWART;" and this he does by taking *sub* for *super*, or deriving it from the Greek preposition ὑπὲρ. I also am confident, that VIRGIL would not have employed in this clause of the sentence *superjaciunt* as synonymous with *subjiciunt*; because the former would have signified, not that *they threw themselves on horseback*, but that *they threw themselves over the horses*; and, besides, the preposition *in*, which, with *subjiciunt*, signifies *upon*, could not have been employed with *superjaciunt* without a gross violation of the idiom of the language*; and no one knew this better than Dr PARR, if he had not been blinded by his theory. The construction would have been the same as the following:

> " pontus
> Nunc ruit ad tellus, scopulosque superjacit undam
> Spumeus." VIRG. *Æn.* XI. 625.

> Ille astu subit, at tremebunda *super*volat hasta.
> VIRG. *Æn.* X. 522.

The quivering spear flies over *him*. The same relation is to be observed in the expression, *Corpora subjiciunt in equos*, as in the former example. The preposition *sub* denotes the *lower* situation of the men relative to the *higher* position of the horses' backs when they were going to throw themselves upon them:

> Ter flamma ad summum tecti subjecta reluxit."
> VIRG. *Georg.* iv. v. 385.

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* I am quite aware that the preposition *in* was sometimes used in compound verbs with *super*; as, *superinjicere*, *superimpono*, &c. But when these two prepositions are combined together, they imply a very different relation. Would VIRGIL, or any other Latin author, have used the expression — *aut corpora saltu* superinjiciunt *equos*? I believe neither he nor any other. When he says, *Georg.* IV. v. 46. " et raras *superinjice* frondes," he shews that the relative situation of the person to the hives is just the reverse of the men to the horses' backs, in the preceding example. Thus, " and *throw* (from a higher situation) *a few branches* down upon *them*."

The *flamma subjecta* has a reference primarily to the *low* situation of the altar on which the fire was burning, compared with the *height* of the roof to which the flame ascended. *Sub*, in this example, in the sense of ὑπὲρ, would have conveyed an extraordinary idea. It would have denoted, *flamed above the roof*, not *up to it;* and with the words *ad summum* would have violated the construction of the language. The same compound frequently denotes *motion under:* and, in such examples, *sub* must evidently be derived from ὑπὸ. Thus, Ovid, *Trist. Eleg.* i. 73.

> " *Canitiem* galeæ *subjicio*que meam."

In the two following instances, quoted by Dr Parr, the same explanation must be given of the preposition.

> " ——— Tibi suaves dædala tellus,
> *Summittit* flores." Lucret. i. v. 9.

Tellus summittit flores, the earth *sends up* flowers; *sub, from her bosom*, which, relatively, is *low* compared with the flowers when they have sprung up.

> " Sic et averna loca alitibus summittere debent
> Mortiferam vim, de terra quæ surgit in auras."
> Lucret. vi. v. 818.

" De terra quæ surgit in auras" explains the whole relation of *summittere* in the preceding line.

Having pointed out the relation indicated by *sub* in composition, in several examples from the poets, I shall now proceed to examine some of the Doctor's examples from prose writers. " In prose writers," says he, " we have *sub* for *up*. " *Sublevare* mentum sinistra," Cicero. "Sublevare miseros," Cicero. The same relation may be observed in both these examples. The chin is raised *from* the breast to a *higher* situation by the left hand. It is *raised up*, and prevented from sinking by the left hand placed *under* it.

The wretched are raised from a *low* to a *higher* (better) condition: *i. e.* a condition *higher* in the scale of existence. Nobody, I suppose, ever heard of *superlevare*. The following quotations will shew the relation which this verb points out. Cicero, *Att.* l. x. c. 17. "Qui nos sibi quondam ad pedes stratos, ne *sublevabat* quidem." Pliny, l. xi. c. 17. "Apes regem fessum humeris *sublevant*." Livy, l. xlv. c. 7. "Consul, introeunti regi dextram porrexit, *submittentem*que se ad pedes *sustulit*."

"Upon *sub*, when standing alone," he says, "I speak doubtfully. "There is a passage in Livy where *subire* may have the sense of "ascending, but I am not positive, and shall offer a different ex-"planation." In the following passage, from the same author, where a description is given of Hannibal's passage over the Alps, the verb *subire* can have no other signification than *to ascend*. "Luce prima subiit tumulos, ut ex aperto et interdiu vim per angustias facturus," l. xxi. c. 32. So also, l. xxvii. c. 18. "Ceterum, quamquam ascensus difficilis erat, et prope obruebantur telis saxisque, assuetudine tamen succedendi muros, et pertinacia animi, subierunt primi."

"Sub," says Dr Parr, "occurs under another form *sus*, which hereafter will be explained. *Sustineo*, "*I hold up*. *Suspicio*, "I look up." Mr Stewart will have the goodness particularly to mark the form *sus*." After some other observations, which it is not necessary to quote here, he proceeds: "Sub, then, signifying "elevation," comes not from ὑπὸ, but from ὑπὲρ, and *sus* does not immediately come from *sub* only, but by another process, as we shall soon see." What immediately follows I shall omit, as of little importance to the argument. He then goes on to say: "Against Scaliger's third position I contend, that *susum* did not come from *sus*, but *versa vice*, (as we ought to say instead of *vice versa*), *sus* comes from *susum*, as *retrovorsum* was contracted into *rursum*, so *supervorsum* was contracted into *sursum*, and *sursum* was softened into *susum*, and *susum* when compounded,

shortened into *sus.*"—Now, I contend that *retrovorsum* was never contracted into *rursum*, but into *retrorsum, backwards;* as being compounded of *retro*, and the perfect participle of *verto* or *vorto*, which was originally *versus, a, um;* and as for *supervorsum*, it is a word of the Doctor's own manufacturing, as it never appears to have been used by any author; at least I can find no traces of it in the best Latin dictionaries. But, even though it had existed, it would have given, by abbreviation, *superorsum* (according to the analogy of *retrorsum)* and not *sursum* *.

The examples which Dr Parr gives of *sus* in composition are, "suscipio," which, he says, is, *Capio susum*, "I take up;" "suspendo," is *susum pendo*, "I hang up;" "sustineo" is *susum teneo*, "I hold up." *Suscito* is, by Scaliger's own confession, *susum cito*, "I stir up;" and as specio begins with an *s*, the final letter of *sus* contracted, (abbreviated, the Doctor should have said, for it is not contracted,) from *susum*, is omitted upon the above mentioned principle of avoiding, as the old Romans avoided, the gemination of the same letter."—It is surprising that Dr Parr did not advert to the practice of the Greeks, in changing the final ν of the prepositions ἐν and σὺν into μ and γ before certain mutes; and also of converting it into whatever liquid the word with which it was joined commenced with. They even omitted the ν of the preposition σὺν before σ and a mute; as συ-στρατένομαι; συ-σπάω, &c. In like manner, I imagine, the *b* of the preposition

* The venerable Dr Hunter of St Andrew's has also shewn, in some notes upon Virgil, that Dr Parr's derivation of *sub* from ὑπὲρ is incorrect. After giving several examples similar to those already quoted, he observes, "It may be further remarked, that sursum, *upward*, is not, as Dr Parr supposes, super*vor*sum, but sub*vorsum*. Super*vorsum* would express, not *in a direction* from *below* or upward, but *in a direction* over." The coincidence of opinion between Dr Hunter and myself, on several points in the present discussion, is to me the more gratifying, as his notes were wholly unknown to me, till communicated by our common friend the Rector of the High School of Edinburgh, after I had sent for his perusal the present examination.

sub was changed into other letters, and sometimes omitted in compound words, euphoniæ causâ.—*Suscipio* is *sub* and *capio*, and so written as being more agreeable to the ear than *subcipio*, or *succipio*, which Scaliger says it originally was. *Sustineo* is for *sub* and *teneo*. In this verb the preposition *sub* expresses more nearly the force of the Greek preposition ἀνὰ than ὑπὸ, as *sustineo* is equivalent to ἀνέχω, "I hold up." Yet, in every example where it occurs, the idea of *supporting under* may be traced, sometimes very plainly, at other times more obscurely. Hence it is often synonymous with *tolero*, *patior*. Thus Ovid. *Metamorph.* viii. v. 500. "Et quos *sustinui* bis mensium quinque labores." Cicero, *Verr.* 8. "Non tibi venit in mentem quid negotii sit, causam publicam sustinere," which Facciolati explains by *portar il peso*. *Suspicio* is from *sub* and *specio*, the *b* being dropped as in the Greek verb συ-σπάω. Its literal meaning is, I look *from under*. Taken in a literal sense, it implies an *higher* object. Thus Cicero, *de Nat. Deor.* ii. c. 2. "Cum cœlum *suspeximus* cœlestiaque contemplati sumus." Metaphorically, the sense of *inferiority*. "Translate *est admirare*," says Facciolati, "ammirare, quasi supra nos aspiciamus illum esse collocatum, quem admiramur." Cicero, *Off.* ii. c. 10. "Itaque eos viros *suspiciunt*, maximisque efferunt laudibus."

From these, and many other examples which could be produced, it may be observed, that the genuine power of the preposition *sub* in composition, is always to mark the relation of *inferiority* to a *higher* object, and that it never can, consistently with the usage of the language, be derived from ὑπὲρ. Had grammarians and philologists been sufficiently attentive to observe the *relations* which prepositions in particular denote, they would not have committed such glaring mistakes as are frequently observable in their account of them. So far as my observation extends, I do not know a single work in which the Greek and La-

tin prepositions are treated of with any thing like philosophical accuracy; and hence the vague and uncertain ideas that are generally entertained of their origin, nature and relations. The classical literature of the day holds a very different course. It contents itself with amassing authorities, investigating different readings, quoting parallel passages, and retailing opinions, without once venturing from the beaten track, to take a view of the principles of language.—The theory of language is no despicable study; for, if well and wisely conducted, it shews the progress of the human mind from a rude state, when it was chiefly conversant with external objects, and ignorant in a great measure of those associations which spring from the view of the living world, and the comparison of its ideas, till it reach, through the medium of relations, more and more refined and abstract in their nature, the highest point of intellectual improvement. From the failure of men of the deepest knowledge, and the greatest powers in the science of philology, I consider it still in its infancy, and likely to remain so, unless, with a thorough knowledge of the primitive languages of Europe and Asia, there be combined more of the study of nature, a better acquaintance with severe inductive reasoning, and the philosophy of the human mind, than has hitherto appeared in the speculations of the learned.

XXVII. *Results of the Thermometrical Observations made at Leith Fort, every Hour of the Day and Night, during the whole of the Years* 1824 *and* 1825. By David Brewster, LL.D. F. R. S. Lond. & Sec. R. S. Ed. Corresponding Member of the Academy of Sciences of Paris, &c.

(Read January 23. 1826.)

In the year 1820, I had occasion to suggest to the Royal Society the propriety of establishing Registers of the Thermometer in various parts of Scotland.

In a country embracing so many varieties of soil, climate and elevation, and extending over nearly six degrees of latitude, it was an object worthy of a public body to determine the Law of the Distribution of Temperature, even if such a subject had not possessed a separate interest in relation to the Horticulture and Agriculture of the Country. The Society did not hesitate in adopting this suggestion, and many intelligent individuals were found, who undertook to observe the thermometer twice a-day, and to measure occasionally the temperature of Springs and Wells. During the first year, viz. 1821, nearly sixty Meteorological Journals were regularly kept in different parts of Scotland. The number diminished considerably in subsequent years; but, notwithstanding this diminution, there is now in our possession a rich series of observations during *five* complete years, the results of which are nearly ready to be submitted to the Society.

In directing these observations, it became necessary to select two hours of the day most convenient for marking the state of the Thermometer, and the Mean Temperature of which approached nearest to the Mean Temperature of the day. The hours adopted were 10 o'clock A. M. and 10 P. M., which had been previously recommended by the Reverend Mr Gordon. The observations were accordingly made at these hours, during three years; but it appeared to me, upon a more attentive consideration of the subject, that the Thermometer should be observed at the two times of the day at which the Mean Temperature occurred; for if one of the observations was omitted, the other still possessed considerable value, as an approximation to the Mean Temperature. Unfortunately, however, there were almost no observations in existence from which the times of the daily Mean Temperature could be deduced. Professor Dewey of New York had observed the Thermometer once every hour during *five* days at a time, in the months of *March*, *April*, *July* and *October* of the year 1816, and during *eight* days of *January*, and *two* of *February* in the year 1817 *; and Mr Coldstream of Leith registered the Temperature of 24 successive hours once every month, from July 1822 to July 1823. From this last series of observations the Mean Temperature appeared to occur at ½ past 7 o'clock in the morning, and ½ past 8 in the evening; and these hours were accordingly used in most of the registers for 1824 and 1825. It was very obvious, however, that these observations, though made with great care, were too limited to afford any thing like an accurate result; and hence it became a desirable object to extend the hourly observations of the Thermometer over a greater portion of the seasons, or, if possible, to record its indications for every hour of a complete year.

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* *Memoirs of the American Academy of Arts and Sciences*, Vol. IV. Part II. p. 392.

As such a plan could only be carried on with effect at a military station, Leith Fort was considered the most eligible. Application was therefore made to Colonel Thackeray, commanding the Engineers, and to Colonel Younghusband and Mr Street of the Artillery; and, as these gentlemen entered warmly into the scheme, preparations were made to begin the register on the 1st of January 1824. A large and accurate thermometer was constructed by Mr Adie for the purpose, and it was placed in a situation as free as possible from all disturbing causes. Its height above the level of the sea is 25 feet, and its distance from the sea 200 yards.

The register commenced on the 1st day of January 1824, and has been regularly and zealously carried on by the non-commissioned officers of the Fort for two complete years. The observations themselves are recorded in the two Folio volumes now submitted to the Society, and they may justly be regarded as possessing a high value, not only from the scientific results which they afford, but as being the only complete series of hourly observations which have been made in any part of the world *.

In reducing these observations, Mr Foggo *junior* of Leith computed all the hourly, monthly, and annual means for the year 1824, and Mr Christopher Bell made the same calculations for 1825. These Mean Results are given in the following Tables.

* Since this was written, we understand that similar observations, suggested by the present series, are now carrying on at Toulon and at Montreal, the last of which will possess a peculiar interest. The first example of this class of observations having been set in Scotland, we may be here allowed to express a hope, that similar hourly registers will be established by learned Societies both in Europe and America.

HOURLY REGISTER FOR 1824,

The Mean Temperature of the Winter months, viz. *Dec. Jan. Feb.* is .... 40°.67
.... of the Spring months, viz. *March, April May,* 45.38
.... of the Summer months, viz. *June, July, Aug.* 57.24
.... of the Autumn months, viz. *Sept. Oct. Nov.* 47.91

The Mean Temperature of the Year 1824, from 8784 observations, is........... 47.81

TABLE I.—*Containing the Daily and Monthly Mean Temperatures for* 1824.

Day.	January.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1	42.55	42.69	35.19	33.49	55.00	57.12	55.85	57.00	62.80	56.00	44.53	32.07
2	43.52	42.69	32.72	39.36	52.25	55.10	57.92	56.86	67.69	51.15	44.80	32.34
3	40.47	42.52	33.77	37.39	47.00	58.64	55.95	57.05	67.78	53.25	39.83	30.92
4	43.79	40.82	35.25	39.50	47.80	56.80	57.97	57.54	60.36	52.72	36.33	28.11
5	44.38	38.69	38.34	43.62	49.84	56.07	55.44	55.89	56.69	51.42	35.87	28.83
6	40.69	40.03	44.52	44.33	46.81	52.63	58.29	55.70	54.10	52.90	35.63	38.54
7	36.50	49.63	43.29	45.06	50.67	60.98	58.75	57.38	58.18	53.79	49.30	38.39
8	45.30	46.40	38.83	44.58	51.98	55.83	63.64	61.37	50.00	54.09	45.76	35.39
9	50.08	41.76	36.50	45 01	51.23	53.00	62.22	58.88	49.96	48.42	44.76	35.39
10	45.67	43.21	35.83	37.92	49.94	51.63	58.18	57.43	51.79	44.39	45.94	34.79
11	39.90	40.04	38.32	37.55	47.38	50.15	58.92	56.79	57.09	46.42	43.32	48.15
12	43.31	43.36	36.82	39.87	47.15	53.87	60.27	55.94	57.00	41.60	39.52	50.28
13	42.26	39.51	36.15	39.27	47.25	55.45	57.75	56.48	57.29	39.03	49.00	48.40
14	39.30	39.37	38.41	40.60	45.25	51.78	66.24	57.22	59.36	44.15	43.23	48.60
15	32.41	34.91	44.22	41.42	47.70	51.86	64.51	58.73	58.39	38.34	35.78	46.10
16	33.37	36.53	46.16	37.88	48.16	53.37	58.81	56.67	60.40	37.44	45.90	38.59
17	38 26	37.65	46.02	41.68	51.34	53.01	58.35	55.58	61.61	37.56	52.15	37.79
18	44.01	38.07	48.00	46.35	47.64	54.45	57.23	54.39	61.78	44.22	42.49	44.09
19	40.56	41.30	45.05	51.52	43.63	53.12	58.13	57.07	54.30	49.98	41.12	49.24
20	41.93	41.07	50.03	54.93	42.50	52.87	61.29	56.03	51.81	48.48	41.52	36.94
21	39.33	41.07	45.34	55.47	44.77	54.03	60.99	53.79	51.06	49.00	41.39	37.53
22	39.32	42.01	38.95	52.27	48.09	55.38	65.73	52.82	56.66	54.50	40.06	35.45
23	38.60	41.51	38.67	48.90	52.77	54.09	63.35	56.10	54.44	53.53	42.64	34.11
24	40.31	41.36	41.37	51.46	54.54	54.57	56.92	58.35	53.44	52.97	45.27	40.33
25	48.04	40.72	42.29	52.64	57.18	56.57	58.72	56.63	50.51	53.96	41.68	46.94
26	51.58	37.83	41.15	53.95	56.75	57.75	59.04	56.67	42.79	48.88	39.12	35.99
27	43.59	38.19	39.99	51.01	59.75	58.78	58.70	55.69	39.70	42.22	36.44	44.39
28	38.69	40.05	39.18	53.58	56.29	59.45	60.53	54.33	40.30	40.28	40.48	39.80
29	36.47	40.73	41.68	55.16	49.62	60.49	61.97	57.62	41.90	38.80	40.04	39.78
30	38.54		37.85	57.90	55.16	60.69	55.37	54.23	55.33	39.17	34.20	41.18
31	45.20		33.86		52.46		56.06	57.01		48.87		46.93
Means,	41.599	40.83	40.12	45.79	50.24	55.65	59.46	56.62	54.57	47.23	41.94	39.57

TABLE II.—*Shewing the Mean Temperature of each Hour for each Month of* 1824, *and for the whole Year.*

Hour.	Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean Temp. of each hour for the whole year.
1 A. M.	41.19	39.9	38.3	42.63	46.56	52.6	55.4	53.3	52.3	46.17	40.4	38.69	45.62
2......	40.8	40.03	38.3	41.6	46.03	52.3	55.2	53.2	52.1	46.17	40.4	38.71	45.40
3......	40.8	39.95	38.07	41.00	45.3	52.1	55.1	52.9	51.4	46.07	40.5	39.02	45.18
4......	40.28	39.68	37.9	40.08	44.7	51.8	54.9	52.5	51.1	46.2	40.47	38.9	44.93
5......	40.07	39.62	37.65	39.8	44.9	51.8	55.2	52.7	51.2	45.6	40.45	38.8	44.82
6......	40.1	39.44	37.45	39.9	45.7	52.7	55.8	53.3	51.6	44.8	40.50	38.9	45.00
7......	40.23	39.27	37.77	42.2	46.9	53.1	56.9	54.5	52.1	45.3	40.7	38.8	45.64
8......	40.3	39.02	38.3	43.1	48.3	54.3	58.2	55.5	53.4	45.9	40.8	38.8	46.32
9......	40.64	39.93	39.13	45.9	49.8	55.2	59.7	56.8	55.0	46.6	41.3	39.0	47.41
10......	41.15	40.74	39.47	47.3	51.1	56.2	60.56	57.9	55.6	47.5	42.1	39.3	48.24
11......	41.54	41.35	41.13	48.2	52.3	57.3	61.5	58.7	56.5	48.5	43.1	40.4	49.21
12......	42.3	42.22	42.23	48.9	53.3	57.8	63.2	59.5	57.5	49.3	43.9	41.0	50.09
1 P. M.	42.83	42.7	42.7	49.3	54.2	58.0	63.2	59.8	58.5	49.9	44.2	41.2	50.45
2......	43.15	42.7	42.8	49.8	54.7	58.9	63.2	60.0	58.7	49.9	44.7	40.9	50.79
3......	43.18	42.67	42.9	50.1	54.7	59.9	63.5	60.0	58.8	49.6	44.7	40.33	50.89
4......	43.00	42.03	42.6	49.9	54.7	59.1	63.6	60.1	57.8	49.07	43.4	39.9	50.43
5......	42.22	41.4	41.9	49.5	54.1	58.7	63.4	59.7	57.8	48.4	42.8	39.72	49.97
6......	41.98	40.9	41.07	49.1	53.2	57.7	62.6	59.1	57.0	47.9	42.4	39.52	49.38
7......	41.7	40.53	40.2	47.8	52.4	56.9	61.7	58.0	55.8	47.2	42.1	39.19	48.64
8......	41.35	40.2	39.6	46.5	50.9	55.7	60.3	56.8	55.07	46.73	41.7	39.00	47.90
9......	41.3	40.2	39.2	45.3	49.4	54.5	58.9	55.9	54.3	46.7	41.3	39.09	47.17
10......	41.26	40.03	38.8	44.6	48.9	53.8	57.6	55.0	53.7	46.0	40.8	39.09	46.64
11......	41.12	39.9	38.3	43.1	47.8	53.2	56.9	54.3	53.4	45.8	40.4	39.1	46.20
12......	40.92	39.9	38.3	42.7	47.1	52.7	56.03	53.8	52.7	45.7	40.3	39.29	45.79

The Mean Temperature obtained from the last column in the above Table, is $47^{\circ}.588$.
It occurred at $9^{h}\ 13'$ A. M. and at $8^{h}\ 26'$ P. M.

HOURLY REGISTER FOR 1825.

The Mean Temperature	of the Winter months, viz. *Dec. Jan. Feb.* is	40.312
....	of the Spring months, viz. *March, April, May,*	46.121
....	of the Summer months, viz. *June, July, Aug.*	59.306
....	of the Autumn months, viz. *Sept. Oct. Nov.*	49.907

The Mean Temperature of the year 1825, from 8760 observations, is........... 48.911

TABLE III.—*Containing the Daily and Monthly Mean Temperatures for* 1825.

Day.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1	45.15	41.31	39.78	42.38	44.71	56.88	55.50	63.77	60.40	53.63	46.35	33.79
2	39.83	41.81	37.29	45.89	46.09	58.41	57.81	63.56	59.19	60.36	42.08	36.85
3	43.47	32.99	36.13	47.46	49.14	53.61	58.54	63.27	59.30	60.03	42.53	34.59
4	37.35	28.72	36.20	49.74	52.73	50.52	59.56	59.60	57.07	58.81	40.36	37.84
5	30.32	33.08	36.50	54.18	48.69	49.48	58.66	59.71	55.39	58.38	43.04	37.09
6	39.26	34.99	38.24	47.26	52.59	52.35	59.24	58.21	55.99	57.57	41.52	40.72
7	44.17	38.51	39.55	49.80	53.19	58.30	58.09	59.06	58.79	53.79	36.04	43.21
8	39.80	38.81	44.20	52.11	54.63	57.46	57.05	59.88	55.45	54.48	36.82	44.71
9	38.11	42.78	52.46	47.04	54.27	55.24	56.05	57.71	58.30	55.49	35.73	42.95
10	35.55	47.11	52.48	48.55	50.77	59.59	55.25	59.37	60.26	56.79	32.37	43.35
11	41.48	46.28	44.97	50.13	48.32	65.67	56.26	57.34	58.21	54.00	36.22	44.12
12	39.91	46.06	44.18	41.03	48.75	62.71	61.09	56.41	60.55	57.80	33.78	43.90
13	42.03	46.98	43.41	40.79	47.50	59.09	65.45	59.74	56.27	54.09	42.32	42.45
14	44.75	41.82	37.83	48.62	48.91	60.14	69.94	56.78	56.30	53.57	40.82	38.56
15	46.73	41.48	36.40	52.50	49.74	58.85	65.51	57.91	56.79	52.51	36.49	41.06
16	43.99	41.79	36.04	50.49	49.05	63.52	66.77	56.88	61.21	54.65	44.12	46.04
17	39.38	43.52	36.75	45.06	51.95	60.51	69.63	57.17	62.40	47.16	44.61	45.40
18	41.22	41.26	39.07	40.54	53.55	58.45	66.63	59.01	62.59	46.75	45.56	47.05
19	39.15	41.78	43.31	42.30	53.41	52.46	62.23	59.07	60.95	46.23	41.14	42.66
20	40.09	43.73	40.58	50.49	50.41	52.70	60.02	65.40	59.87	40.24	45.56	42.44
21	39.30	40.91	39.78	53.04	49.01	52.09	59.07	64.35	60.34	41.47	45.22	45.92
22	37.02	42.63	42.55	46.81	52.14	53.16	59.83	60.41	54.95	42.47	40.76	40.02
23	36.33	40.88	40.61	42.76	50.48	57.68	56.63	65.44	49.44	51.07	44.74	40.22
24	38.03	40.17	38.12	42.37	46.01	57.59	57.10	57.88	63.49	48.97	43.30	39.75
25	34.45	39.08	42.03	42.80	45.60	57.14	63.29	58.04	62.16	40.65	40.46	43.85
26	41.30	37.69	42.81	45.28	48.55	52.44	61.01	57.23	56.29	39.26	44.80	35.14
27	47.65	38.61	48.70	45.88	47.02	54.35	64.94	58.45	56.59	45.58	35.81	33.00
28	38.83	36.75	45.39	47.35	45.18	54.70	60.22	59.21	53.00	52.41	36.44	35.86
29	43.88		47.47	47.88	47.96	55.95	61.05	61.92	55.45	50.98	39.84	35.01
30	47.62		44.78	48.51	50.17	55.05	68.53	65.35	54.66	51.90	34.63	32.81
31	42.93		42.29		52.74		68.20	65.72		46.81		28.58
Mean Temp. of each month,	40.583	40.412	41.610	46.968	49.785	56.531	61.262	60.125	58.055	51.223	40.443	39.940

TABLE IV.—*Shewing the Mean Temperature of each Hour for each Month in* 1825, *and for the whole Year.*

Hour.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean Temp. of each hour for the whole year.
1 A. M.	39.073	39.437	39.604	43.350	47.016	52.575	56.935	57.355	55.358	49.984	39.325	39.194	46.648
2......	39.968	39.500	39.162	42.742	46,750	52.200	56.798	56.936	55.058	49.782	38.800	39.331	46.466
3......	39.823	39.598	38.516	42.050	46.460	52.133	56.087	56.661	54.792	49.685	38.975	39.105	46.198
4......	39.855	39.509	38.355	41.500	46.161	51.975	55.403	56.629	54.333	49.476	38.858	39.032	45.969
5......	39.814	39.089	38.275	41.317	46.210	52.067	56.121	56.565	53.992	49.516	39.050	39.016	45.968
6......	39.774	39.018	38.404	41.542	46.944	52.958	57.637	56.758	54.117	49.484	39.183	39.234	46.307
7......	39.831	39.321	38.718	43.050	47.476	54.150	58.855	57.542	55.192	49.797	39.417	39.194	46.927
8......	39.927	39.518	39.524	44.725	48.597	55.250	60.032	58.887	56.442	50.451	39.383	39.468	47.738
9......	40.121	39.589	40.661	46.842	49.653	56.883	61.298	59.960	58.050	51.137	39.825	39.718	48.700
10......	40.508	40.473	41.605	48.892	50.702	58.117	62.693	61.170	59.342	52.484	40.825	40.089	49.784
11......	41.161	41.670	42.701	49.992	51.363	59.017	63.500	61.895	60.667	53.258	41.833	40.645	50.691
12......	41.748	42.241	44.404	51.142	51.863	59.508	64.484	63.008	61.500	53.766	42.258	41.016	51.464
1 P. M.	42.000	42.848	44.661	51.617	52.186	60.050	64.670	63.629	62.100	54.008	42.667	41.169	51.848
2......	42.032	42.821	45.226	51.817	52.540	60.500	65.468	63.826	62.575	54.137	42.708	41.532	52.150
3......	42.009	42.928	45.259	52.275	52.686	60.533	65.798	63.830	62.258	53.855	42.808	41.250	52.174
4......	41.371	42.482	45.484	51.858	53.315	60.517	65.807	64.210	62.075	53.193	42.250	41.105	52.049
5......	41.153	41.562	45.210	51.175	53.645	60.450	66.250	64.387	62.042	52.557	41.517	40.652	51.774
6......	40.920	41.089	44.630	50.767	53.493	60.591	66.742	64.387	60.333	51.613	41.192	40.355	51.208
7......	40.475	40.705	43.556	49.275	52.484	59.867	65.975	61.927	58.875	51.065	40.667	39.798	50.449
8......	40.404	40.250	42.711	47.300	51.137	57.608	62.807	60.589	58.008	50.477	40.442	39.758	49.348
9......	40.258	39.625	41.718	46.117	49.637	56.242	60.742	59.476	57.283	50.202	40.350	39.532	48.488
10......	40.395	39.286	41.083	45.425	49.097	55.167	59.492	58.409	56.575	50.129	39.867	39.411	47.913
11......	40.283	39.170	40.523	44.733	48.508	54.225	58.581	57.779	56.042	49.742	39.492	39.153	47.407
12......	40.339	39.143	40.242	44.133	47.927	53.675	57.605	57.234	55.517	49.670	39.117	38.855	47.007

The Mean Temperature obtained from the last column of the above Table is 48°.944. It occurred at 9^h 13′ A. M. and 8^h 28 P. M.

MEAN RESULTS OF THE HOURLY REGISTER FOR 1824 AND 1825.

The Mean Temperature of the Winter months, viz. *Dec. Jan. Feb.* is....................40°.491
.... of the Spring months, viz. *March, April, May,*....................45.751
.... of the Summer months, viz. *June, July, Aug.* 58.273
.... of the Autumn months, viz. *Sept. Oct. Nov*......................48.908

The Mean Temperature of the years 1824 and 1825, from 17,544 observations, is......48.360

TABLE V.—*Containing the Average of the Daily and Monthly Temperatures of the Years* 1824 *and* 1825.

Day.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.
1	43.85	42.00	37.48	37.93	49.85	57.00	55.67	60.38	61.60	54.82	45.44	32.93
2	41.67	42.25	35.01	42.62	49.17	56.75	57.86	60.21	63.44	55.75	43.44	34.59
3	41.47	37.75	34.95	42.42	48.07	56.12	57.23	60.16	63.54	56.64	41.18	32.75
4	40.57	34.77	35.72	44.62	50.26	53.66	58.76	58.57	58.71	55.76	38.34	32.97
5	37.35	35.88	37.42	48.90	49.26	52.75	57.05	57.80	56.04	54.90	39.45	32.96
6	39.97	37.51	44.38	45.79	49.70	52.49	58.76	56.95	55.04	55.23	38.58	39.63
7	40.33	44.07	41.42	47.43	51.93	59.64	58.42	58.22	56.48	58.79	42.67	40.80
8	42.55	44.61	41.52	48.34	53.30	56.64	60.35	60.62	52.72	54.29	41.29	40.05
9	44.09	42.27	44.48	46.03	52.75	54.12	59.13	58.29	54.13	51.95	40.24	39.17
10	40.61	45.16	44.15	43.23	50.35	55.61	56.71	58.40	56.02	50.59	39.15	39.07
11	40.69	43.16	41.64	43.84	47.85	57.91	57.59	57.07	57.65	50.21	39.77	46.13
12	41.61	44.71	40.50	40.45	47.95	58.29	60.68	56.17	58.77	49.70	36.62	47.09
13	42.15	43.24	39.78	40.03	47.37	57.27	61.60	58.11	56.78	46.56	45.66	45.42
14	42.02	40.59	38.12	44.61	47.08	55.96	68.09	57.00	57.83	48.86	42.02	43.58
15	39.57	38.19	40.31	46.96	48.72	50.35	65.01	58.32	57.59	45.42	36.13	43.58
16	38.68	39.16	41.10	44.19	48.60	58.44	62.79	56.77	60.80	46.04	45.01	42.31
17	38.82	40.58	41.38	43.37	51.64	56.26	63.99	56.37	62.01	42.36	48.38	41.59
18	42.61	39.66	43.53	43.45	50.59	56.45	61.93	56.70	62.18	45.48	44.02	45.57
19	39.85	41.54	44.18	46.91	48.52	52.79	60.18	58.07	57.62	48.10	41.13	45.95
20	44.01	42.40	45.30	52.71	46.45	52.78	60.65	60.72	55.84	44.36	43.54	39.69
21	39.31	40.99	42.56	54.25	46.81	53.06	60.03	59.07	55.70	45.23	43.30	41.72
22	38.16	42.32	40.75	49.54	50.11	54.27	62.78	56.61	55.81	48.48	40.41	37.73
23	37.46	41.19	39.64	45.83	51.62	55.88	59.99	60.77	51.94	52.30	43.69	37.16
24	39.17	40.76	39.74	46.92	50.27	56.06	57.01	58.11	58.46	50.97	44.28	40.04
25	41.24	39.90	42.16	47.72	51.39	56.85	61.01	57.33	56.33	47.30	41.05	45.39
26	46.44	37.76	41.98	49.61	52.65	55.09	60.02	56.95	49.54	44.07	41.96	35.56
27	45.62	38.40	44.34	48.45	53.39	56.56	61.82	57.07	48.14	43.90	36.12	38.69
28	38.76	38.40	42.28	50.46	50.73	57.07	60.37	56.77	46.65	46.34	38.46	37.83
29	40.18		44.57	51.52	48.79	58.22	61.51	59.77	48.67	44.89	39.94	37.39
30	43.08		41.31	53.20	52.66	57.87	61.95	59.79	54.99	45.53	34.42	36.99
31	44.06		38.07		52.60		62.13	61.36		47.84		37.75
Means,	41.091	40.621	40.865	46.379	50.012	56.091	60.361	58.372	56.312	49.226	41.191	39.775

TABLE VI.—*Shewing the Average Mean Temperature of each Hour for each Month in 1824 and 1825, and the average of these Two Years.*

Hour.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean Temp. of each hour for the whole two years.
1 A. M.	40.131	39.668	38.952	42.990	46.788	52.587	56.167	55.327	53.829	48.077	39.862	38.942	46.134
2	40.384	39.751	38.731	42.171	46.390	52.250	55.999	55.068	53.579	47.976	39.600	39.020	45.933
3	40.311	39.774	38.293	41.525	45.880	52.116	55.593	54.780	53.096	47.877	39.737	39.062	45.689
4	40.067	39.594	38.127	40.794	45.430	51.887	55.152	54.564	52.716	47.838	39.664	38.966	45.449
5	39.942	39.354	37.962	40.558	45.550	51.933	55.660	54.632	52.596	47.558	39.750	38.908	45.394
6	39.932	39.229	37.927	40.721	46.322	52.829	56.718	55.029	52.858	47.142	39.841	39.062	45.653
7	40.030	39.295	38.244	42.625	47.188	53.625	57.877	56.021	53.646	47.548	40.058	38.997	46.283
8	40.113	39.269	38.912	43.912	48.449	54.775	59.116	57.193	54.921	48.175	40.092	39.134	47.029
9	40.381	39.759	39.895	46.371	49.726	56.041	60.499	58.380	56.525	48.868	40.562	39.359	48.055
10	40.829	40.606	40.537	48.096	50.901	57.158	61.626	59.535	57.470	49.992	41.462	39.694	49.012
11	41.350	41.510	42.915	49.096	51.830	58.158	62.500	60.297	58.583	50.879	42.466	40.522	49.950
12	42.024	42.230	43.317	50.021	52.581	58.654	63.842	61.254	59.500	51.533	43.079	41.008	50.777
1 P. M.	42.415	42.774	43.680	50.458	53.193	59.025	63.935	61.714	60.300	51.954	43.433	41.184	51.149
2	42.591	42.761	44.013	50.808	53.620	59.700	64.334	61.913	60.637	52.018	43.704	41.216	51.470
3	42.594	42.799	44.079	51.187	53.693	60.216	64.649	61.915	60.529	51.727	43.754	40.790	51.532
4	42.185	42.256	44.042	50.879	54.007	59.808	64.703	62.155	59.937	51.132	42.825	40.502	51.239
5	41.686	41.481	43.555	50.337	53.872	59.575	64.825	62.043	59.921	50.478	42.158	40.186	50.872
6	41.450	40.994	42.850	49.933	53.346	59.145	64.671	61.743	58.666	49.756	41.796	39.937	50.294
7	41.082	40.617	41.878	48.537	52.442	58.383	63.837	59.963	57.337	49.132	41.383	39.494	49.544
8	40.877	40.225	41.155	46.900	51.619	56.654	61.553	58.694	56.539	48.603	41.071	39.379	48.624
9	40.792	39.913	40.459	45.708	49.518	55.371	59.821	57.688	55.791	48.451	40.825	39.311	47.829
10	40.827	39.658	39.941	45.012	48.998	54.483	58.546	56.704	55.137	48.064	40.333	39.250	47.276
11	40.702	39.535	39.411	43.916	48.154	53.712	57.740	56.039	54.721	47.771	39.941	39.126	46.803
12	40.629	39.521	39.271	43.416	47.513	53.187	56.817	55.517	54.109	47.635	39.708	39.072	46.398

The Mean Temperature obtained from the last columns of the preceding Table is 48°.266. It occurred at 9^h 13′ A. M. and 8^h 27′ P. M.

Having given, in the preceding Tables, all the numerical results of the hourly Register for 1824 and 1825, I shall now proceed to consider some of the most important conclusions which may be deduced from them. These relate,

1. To the form and character of the annual and monthly daily curve, or the daily progression of temperature.
2. To the determination of the two times of the day when the mean temperature occurs.
3. To the relation between the mean temperature of the 24 hours, and that of any single hour, or pair of similar hours, &c.
4. To the average daily range for each month.
5. To the Parabolic form of the branches of the Annual Daily Curve.

1. *On the Form and Character of the Annual and Monthly Daily Curve, or the Daily Progression of Temperature.*

The daily curve for 1824 is projected in Plate XIV., from the numbers in the last column of Table II., and forms the lowest curve. The point of the curve for each of the 24 hours is the mean of 366 observations. The temperature is lowest between 4 and 5 o'clock in the morning; it then increases with great regularity till three o'clock in the afternoon, when it descends till it reaches its minimum at 5 o'clock in the morning. The period during which it performs its ascending motion is $9^h\ 40'$, and the period of its descending motion is $14^h\ 20'$; the heat of the day, therefore, advances with more rapidity than the cold of the night.

The daily curve of 1825 is projected in a similar manner in Plate XIV. from the first column of Table IV., and forms the

upper curve of the Plate. Each point of it is the mean of 365 observations. Its resemblance and general parallelism to that of 1824, cannot fail to strike the reader, and proves how nearly these observations have conducted us to the form of the daily curve.

The intermediate curve, which is laid down from the last column of Table VI., and is the mean of the two curves, is nearly free from the very slight inequalities in the afternoon branch of both curves, and may be considered as representing with great accuracy the mean annual daily curve for the Latitude of Leith, and at the level of the sea.

In order to observe the variation in the form of the daily curves for different seasons, I have given in Plate XV. and XVI. their projections for every month in 1824 and 1825; and in Plate XVII. the mean of the monthly curves in 1824 and 1825. These curves obviously divide themselves into *three* groupes or classes, viz. 1. The *Curves of High Temperature*, such as those of *June*, *July*, *August* and *September*; 2. The *Curves of Low Temperature*, such as those of *November*, *December*, *January*, *February*, *March*; and, 3. The *Curves of Moderate Temperature*, such as those of *April*, *May*, and *October*.

But though the curves thus group themselves into three varieties of temperature, yet it is obvious that those of April and May have the same form as those of the summer months, while the curve of October resembles the flat curves of the winter months. In order, therefore, to obtain a type of the daily curves of summer and winter, I have joined October to the winter months, and April and May to the summer ones, as in the following Table for 1824 and 1825 *.

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* It is probable that the Royal Society of Edinburgh will publish a series of Plates representing the daily curve for each day of 1824 and 1825.

TABLE, shewing the Mean Temperature of each Hour for Six Summer Months, from April to September inclusive, and for Six Winter Months from October to March inclusive.

Hours.	Six Summer Months.	Six Winter Months.
1 A.M.	51.27°	40.77°
2	50.90	40.91
3	50.50	40.84
4	50.09	40.70
5	50.15	40.57
6	50.77	40.52
7	51.83	40.69
8	53.06	40.94
9	54.59	41.46
10	55.80	42.19
11	56.74	43.27
12	57.64	43.86
1 P.M.	58.10	44.24
2	58.50	44.38
3	58.70	44.29
4	58.58	43.83
5	58.43	43.26
6	57.92	42.80
7	56.75	42.26
8	55.22	41.88
9	53.98	41.62
10	53.15	41.34
11	52.38	41.08
12	51.91	40.97
Mean,	54.59	40.63

By means of this Table I have projected the summer and winter curves, as in Plate XVIII. These curves are beautifully regular, and may be regarded as an accurate type of the daily progression of temperature in summer and winter, each point of each curve being the mean of about 180 observations.

The summer curve descends regularly from midnight till 4 o'clock in the morning, when the coldest time of the day occurs, and it ascends with great regularity till 3 o'clock, when it commences a very rapid descent to its minimum; the total mean range being about 8°.61.

The winter curve, on the contrary, has a gentle rise from 1 A. M. till 2 A. M. It then descends till 6, when it commences its ascent, reaches its maximum at 2, and again descends, but more slowly than it rose, the greatest difference of temperature being about 3°.86. By examining the individual curves which compose the winter group, especially in the means of 1824 and 1825, the rise of temperature after midnight, and its subsequent fall, will be very apparent,—an effect which never takes place in the curves of summer.

The difference of character in the curves of April and October deserves to be noticed. Although these months are considered as giving nearly the mean of the year, and therefore as resembling each other in temperature, yet there is a singular difference in the mode of its distribution. In October the mornings and evenings are comparatively warm, while in April these times of the day are remarkably cold. April, in short, unites the low temperature of a winter month, with the great range of a summer month; while October unites the temperature of a summer month, with the low range of a winter one.

II. *On the Determination of the two times of the Day, when the Mean Temperature occurs.*

I am not aware of any observations made in our climate, by which the hours, when the mean temperature of the day occurs, could be determined. It has generally been believed that it occurs at 8 o'clock in the morning; and Professor PLAYFAIR not

only considers this as nearly the hour of mean temperature for Edinburgh, but he regards the maximum as occurring "from 1 to half past 2, or even 3 o'clock *;" and upon these principles he has selected his three hours, viz. 8 A. M., the time of maximum, and 10 o'clock P. M.

It appears, however, from Tables II., IV. and VI. that the mean temperature of the 24 hours occurs at the following times:

	Hours of Morning Mean Temperature.	Hours of Evening Mean Temperature.
	H. ′	H. ′
1824,	9 13	8 26
1825,	9 13	8 28
Mean of two years,	9 13	8 27

This very extraordinary agreement between the results of 1824 and 1825, shews how nearly we have approximated to the true form of the daily curve, and how much confidence may be placed in the general result. The following may therefore be regarded as the leading points of the annual daily curve.

	H. ′	
Time of Minimum Temperature, a little before -	5 0	A. M.
Time of the Morning Mean Temperature, -	9 13	A. M.
Time of Maximum Temperature, - -	2 40	P. M.
Time of Evening Mean Temperature, - -	8 27	P. M.
Interval between Minimum and following Maximum,	9 40	
Interval between Maximum and following Minimum,	14 20	
Interval between Morning and Evening Mean, -	11 14	
Interval between Minimum and Morning Mean, -	4 13	
Interval between Evening Mean and following Minimum,	8 33	

* *Edinburgh Transactions*, vol. iv. p. 214, and vol. v. p. 293, 294. Mr PLAYFAIR also adds, that 10 o'clock P. M. is " as near as circumstances will allow to the time of greatest cold." These opinions prove how little was then known of the form of the daily curve. It will be seen from Plate XVIII. that, both in the winter and the summer curve, the maximum temperature is never before 3 o'clock, and that 10 o'clock at night is nearer the mean than the minimum temperature of the day.

The determination of the exact times of mean temperature throughout the year, furnishes us with the two best times of the day for recording the indications of the thermometer. These times at Leith are obviously $9^h\ 13'$ A. M. and $8^h\ 27'$ P. M.; for, if any of the observations is accidentally omitted at one of the hours, the mean of the remainder will approach nearer to the mean temperature of the year, than if any other two hours had been taken, and similar omissions made.

There is, however, another advantage of this determination, namely, that the mean temperature of the year may be obtained with great accuracy by a single observation made every day at one of the times of mean temperature. Let us suppose that we wished to determine the mean temperature of the year 1825 at Leith, and that we had possessed no other observations than a single daily one made at $9^h\ 13'$, then the mean of these 365 daily ones would have been $48^\circ.944$, the very same result that has been obtained in 1825 from 24 observations every day.

If we examine the annual curve, and also the monthly curve, it will be seen, that the ascending or morning branch is more regular in its progression than the descending or evening branch. On this account, *we would prefer a single observation every day, made at the time of the morning mean*, to a single observation made every day at the time of the evening mean.

It must be carefully observed, that the hours of mean temperature which we have now been considering, are only mean results for the whole year. If we wished to deduce the mean monthly temperatures from an observation made once a-day, it would not answer to take $9^h\ 13'$ A. M. and $8^h\ 27'$ P. M.; for the times of mean monthly temperature occur at different hours of the day throughout the year, as will appear from the following Table:

TABLE, shewing the Hours of the Morning and Evening when the Mean Monthly Temperature occurs.

	1824.		1825.		Mean of 1824 & 1825.	
	A. M.	P. M.	A. M.	P. M.	A. M.	P. M.
	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
January,	11 4	7 17	10 7	6 37	10 34	6 57
February,	10 9	6 11	9 56	7 40	10 2	6 56
March,	10 19	7 8	10 1	9 9	10 10	8 8
April,	8 58	8 35	9 4	8 17	9 1	8 26
May, -	9 21	8 26	9 7	8 54	9 14	8 40
June, -	9 27	8 2	8 47	8 47	9 7	8 24
July, -	8 51	8 36	8 59	8 45	8 55	8 40
August,	8 51	8 13	9 8	8 25	9 0	8 19
September,	8 44	8 39	9 0	7 57	8 52	8 18
October,	9 46	6 54	9 4	6 42	9 25	6 48
November,	9 56	7 21	9 23	8 0	9 39	7 41
December,	10 15	5 45	9 37	6 45	9 56	6 15

The following are the results for the six winter months, from October to March inclusive, and for the six summer months, from April to September inclusive:

	1824.		1825.		Mean of 1824 & 1825.	
	H. ′	H. ′	H. ′	H. ′	H. ′	H. ′
Six Summer months,	9 2	8 25	9 1	8 31	9 1½	8 28
Six Winter months,	10 12	6 49	9 42	7 29	9 57	7 9

III. *On the relation between the Mean Temperature of the 24 Hours and that of any single Hour, or any similar Pair of Hours, &c.*

It was long the practice of meteorologists to observe the thermometer three times a-day, on the supposition that the mean of these three observations gave the mean temperature of the 24 hours. Observations of this kind are still continued in many

parts of Europe. To the following short Table of some of these, I have added the deviations from the mean temperature, as computed from the results of the preceding Tables:

	Morning.	Afternoon.	Night.	Deviation from Mean Temp. of day.	
Edinburgh, -	8h	*Maximum.*	10h	+ 0°.346	Professor Playfair.
Williamstown,	7	2	9	+ 0.510	Professor Dewey.
	8	1	6	+ 1.225	Proposed by the Phil. Soc. of New York.

As three daily observations are not convenient for many meteorclogists, who are engaged in professional pursuits, it became desirable to select those two hours, the mean of whose temperatures approached nearest to that of the whole day. The following times have been used in this country, and many of them give results that differ very considerably from the mean temperature of the 24 hours:

	Morning.	Afternoon.	Deviation from Mean Temp. of Day.
Hawkhill, - -	8	2	+ 0.982
Gordon Castle, - -	8	2	+ 0.982
Kinfauns, - -	8	10	—1.114
Ditto, - - -	10	10	—0.122
Leadhills, - -	6	1	—0.134
Isle of Man, - -	9	11	—0.838
Royal Society, London, -	9	3½	+ 1.453
......	9	3	+ 1.526
......	9	2½	+ 1.511
......	8½	3	+ 1.273
......	8½	2½	+ 1.258
......	8	3	+ 1.013
......	8	2	+ 0.982
......	7	3	+ 0.641
......	7	2	+ 0.610
Royal Society, Edinburgh,	10	10	—0.120
......	7½	8½	—0.805
......	9¼	8½	0.000

I have given these examples principally with the view of shewing the application of the results of the hourly register, and not with the design of contrasting the hours employed by different observers; for it yet remains to be determined how far the form and dimensions of the daily curve, as determined for Leith, are applicable to places in different latitudes, and situated at different heights above the sea. At Paris, for example, the mean temperature of the day occurs before 9 o'clock in the morning; and at Tweedsmuir * in Scotland, 1300 feet above the sea, it happens before $7\frac{1}{2}^h$ A.M.; but it must be remarked, that the observations at 9 o'clock in the one case, and at $7\frac{1}{2}^h$ in the other, are compared with a calculated mean temperature, and not with the mean temperature of the whole 24 hours †.

It is curious to remark, that, with the exception of the hours of 10 A.M., and 10 P.M., no similar pair of hours has been used by meteorologists. The following Table will shew how nearly at Leith the mean of every similar pair of hours approaches to the mean temperature of the day.

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* At Salem, Massachussets, where a very accurate register has been kept by Dr Holyoke for twenty-six years, the morning mean temperature always occurs before 8 o'clock in the morning.

† According to a very accurate register kept by Mr FAIRLIE, schoolmaster of this parish, the results for 1825 are,

Mean Temp. at $7\frac{1}{2}^h$ A.M.	Mean Temp. at $8\frac{1}{2}^h$ P.M.	Mean Temp. deduced from these.
45°.167	43°.825	44°.494

TABLE, showing the Deviations of the Mean Temperature of every similar Pair of Hours from that of the Day.

Hours.		1824.	1825.	Mean.
5 A.M. ...	5 P.M.	—0°.193	—0°.073	—0°.133
6 ...	6	—0.398	—0.187	—0.292
7 ...	7	—0.448	—0.256	—0.352
8 ...	8	—0.478	—0.401	—0.440
9 ...	9	—0.298	—0.350	—0.324
10 ...	10	—0.148	—0.096	—0.122
11 ...	11	+0.117	+0.105	+0.111
12 ...	12	+0.352	+0.286	+0.319
1 ...	1	+0.447	+0.301	+0.374
2 ...	2	+0.507	+0.364	+0.435
3 ...	3	+0.447	+0.242	+0.345
4 ...	4	+0.092	+0.065	+0.079

From this Table it follows,

1. That of all the similar pair of hours, the mean of 4^h and 4^h approaches nearer to the mean temperature of the day than any other pair.

2. That the deviation of any pair is *less than half a degree of Fahrenheit's scale.*

3. That the mean temperature of the pairs from 5^h to 11^h are less than the mean temperature of the day; and that the mean temperature of the pairs from 11^h to 5^h *exceed* the mean temperature of the day *.

* It now became interesting to compare with these results those deduced from Mr Coldstream's hourly register for one day in each month of the year, and also those obtained by Professor Dewey at Williamstown in North America.

In some instances, meteorological registers have been kept, in which the thermometer has been observed only once a-day. These registers may now be rendered useful, by means of the

Results deduced from Mr COLDSTREAM'*s Observations in* 1822 *and* 1823.

Hours.	Deviations from the Mean Temp. of the day.
5 and 5	+ 0°.005
6 ... 6	—0.445
7 ... 7	—0.574
8 ... 8	—0.580
9 ... 9	—0.674
10 ... 10	—0.207
11 ... 11	—0.170
12 ... 12	—0.106
1 ... 1	+ 0.385
2 ... 2	+ 0.385
3 ... 3	+ 0.840
4 ... 4	+ 1.018

The law of the deviations in this table is very regular, particularly when we consider that the observations were made only on twelve days in the year.

Results deduced from Professor DEWEY'*s Observations.*

Hours.	MARCH 1816.	APRIL.	JULY.	OCTOBER.	JAN. 1817.
5 and 5	+ 0°.64	—0°.41	—0°.49	—1°.77	—1°.15
6 ... 6	—0.76	—1.48	—1.03	—2.85	—1.51
7 ... 7	—1.39	—1.26	—0.91	—3.81	—1.91
8 ... 8	—1.44	—0.87	—0.92	—2.95	—1.55
9 ... 9	—1.96	—0.44	—0.60	—0.64	—0.35
10 ... 10	—1.38	—0.28	—0.89	+ 0.86	+ 1.44
11 ... 11	—0.62	+ 0.97 *	—0.15	+ 2.60	+ 3.47
12 ... 12	+ 0.69	+ 0.07	+ 1.48	+ 1.82	+ 0.31
1 ... 1	+ 1.35	+ 1.02	+ 1.48	+ 2.10	+ 0.67
2 ... 2	+ 2.03	+ 1.23	+ 1.56	+ 2.45	+ 0.77
3 ... 3	+ 1.68	+ 0.74	+ 1.47	+ 1.91	+ 0.44
4 .. 4	+ 1.02	+ 0.57	—0.21	+ 0.28	—0.12

* There is an error in Professor Dewey's mean of 11 P. M.: it should be 34°.92, in place of 38°.92.

following Table, which shews the relation between the mean temperature of each hour and that of the whole day:

Hour.	1824.	1825.	Mean of 1824 & 1825.
1 A. M.	—1.97	—2.296	—2.133
2	—2.19	—2.478	—2.334
3	—2.41	—2.746	—2.578
4	—2.66	—2.975	—2.818
5	—2.77	—2.976	—2.873
6	—2.59	—2.637	—2.613
7	—1.95	—2.017	—1.983
8	—1.27	—1.206	—1.238
9	—0.18	—0.244	—0.212
10	+0.65	+0.840	+0.745
11	+1.62	+1.747	+1.683
12	+2.50	+2.520	+2.510
1 P. M.	+2.86	+2.904	+2.882
2	+3.20	+3.206	+3.203
3	+3.30	+3.230	+3.265
4	+2.84	+3.105	+2.972
5	+2.38	+2.830	+2.605
6	+1.79	+2.264	+2.027
7	+1.05	+1.505	+1.277
8	+0.31	+0.404	+0.357
9	—0.42	—0.456	—0.438
10	—0.95	—1.031	—0.990
11	—1.39	—1.537	—1.463
12	—1.80	—1.937	—1.868

From this table, it appears, that the mean annual temperature of any hour of the day never differs more than $3^{\circ}\frac{1}{5}$ from the mean temperature of the day for the whole year. It deserves also to be noticed, that the deviations in the year 1825 are *uniformly greater* than those in 1824, which no doubt arises from the former having been a much warmer year than the latter.

In order to obtain the mean temperature of the year from a register which contains observations made only once every day, we have only to correct the mean temperature which the regis-

ter gives, by applying, according to its sign, the correction opposite to the given hour. In place of taking the mean of the two years, it might be preferable to take the results for 1824 in cold years, and those for 1825 in warm years.

Before concluding this part of the subject, it may be interesting to ascertain, from the preceding results, the relation between the mean temperature of the day, and the results obtained from the hours used at Paris, Halle, and Abo, where the thermometer is observed more than three times:

Hours used:

	Morning.	Noon.	Afternoon.	Evening.		Deviation from the Mean Temp.
Paris *, -	9	12	3	9		+ 1°.282
Halle, -	8	12	2	6	10	+ 1.103
Abo, -	8	11	2	5	10	+ 1.053

These deviations are very great, and shew how little is gained by multiplying observations, as in the preceding journals. Any two pair of similar hours would have given deviations less than one-third of those in the preceding table. Indeed, it is obvious, that any number of observations made during the day, can never give a correct mean, without corresponding observations made late at night and early in the morning. In the register at Paris, Halle, and Abo, it would require the addition of the minimum to obtain the proper mean, as appears from the following Table:

	Morning.	Noon.	Afternoon.	Evening.		Deviation from the Mean Temp.
Paris, *Minimum*,	9	12	3	9		+ 0°.451
Halle, *Minimum*,	8	12	2	6	10	+ 0.441
Abo, *Minimum*,	8	11	2	5	10	+ 0.399

* The hours of 9 and 9 being nearly those that give the mean temperature of the day, it is obvious, that the mean of 12 and 3, must give a result considerably above the mean temperature of the day, and consequently, that the mean of all the *four* observations must err considerably in excess.

Even with the addition of the minimum, it appears that the mean temperature of these hours errs in excess.

IV. *On the average Daily Range for each Month.*

In a climate so variable as that of Scotland, the daily range of the thermometer is often very great, both in winter and in summer; but the average daily range which we propose now to notice, is the measure of the daily change of temperature for each month, and will of course bear some relation to the sun's declination, as appears from the following Table:

TABLE, showing the average Daily Range of Temperature for each Month, and for the whole Year.

	1824.			1825.			Mean of 1824 & 1825 *.		
	Hour of Min.	Hour of Max.	Daily Range.	Hour of Min.	Hour of Max.	Daily Range.	Hour of Min.	Hour of Max.	Daily Range.
	H.	H.	°	H.	H.	°	H.	H.	°
January,	5	3	3.11	6	2	2.258	6	3	2.662
February,	8	1½	3.68	6	3	3.910	6	3	3.570
March,	6	3	5.45	5	4	7.209	6	3	6.152
April,	5	3	10.3	5	3	10.958	5	3	10.629
May,	4	3	10.0	4	5	7.484	4	4	8.577
June,	4½	3	8.1	4	3	8.558	4	3	8.263
July,	4	4	8.7	4	4	10.404	4	5	9.673
August,	4	4	7.5	5	5½	7.822	4	4	7.591
September,	4	3	7.6	5	2	8.573	5	2	8.041
October,	6	1½	5.1	4	2	4.661	6	2	4.876
November,	12	2½	4.4	2	3	4.008	2	3	4.154
December,	7½	1	2.4	12	2	2.677	5	2	2.308
Whole year,	5	3	6°.07	5	3	6°.06	5	3	6°.138

* This column is computed from Table VI. and is not the mean of the two preceding columns.

From the general character of the year 1825, these results, as might have been expected, present greater uniformity in that year than they do in 1824, or even in the mean of the two years. The mean range is nearly at its maximum about the winter solstice, and gradually increases till April, when it reaches its maximum. It then declines, and again rises to a second maximum in July, after which it gradually diminishes till the end of the season. The mean range for the year is 6°.065, and does not vary above the 100th part of a degree in 1824 and 1825.

V. *On the Parabolic form of the different branches of the Mean annual Daily Curve.*

Before concluding this Report, I was desirous of ascertaining if the different branches of the daily curve had a resemblance to any known curve. Their similarity to the parabola is very obvious, from Fig. 2. of Plate XIV. where they are distinctly projected; and I therefore calculated the following Table, upon the supposition that AB, BC, CD, and DE, were parabolic branches of the following dimensions:

Branch AB,	Ordinate,	AH = 513	
	Abscissa,	BH = 172	or 2°.872
Branch BC,	Ordinate,	CH = 253	
	Abscissa,	BH = 172	or 2°.872
Branch CD,	Ordinate,	CG = 347	
	Abscissa,	DG = 196	or 3°.266
Branch DE,	Ordinate,	EG = 327	
	Abscissa,	DG = 196	or 3°.266

The ordinates 513 + 253 + 347 + 327 are = 1440′ = 24 hours; and the abscissa BH = 2°.872, and DG = 3°.266, when reduced to the same scale as that of the ordinates, become 172

and 196′, as one degree of temperature on the projection is equal to one hour. The abscissæ which represent the temperature were reconverted into degrees.

TABLE, shewing the Mean Annual Hourly Temperature for 1824 *and* 1825, *as observed, and calculated on the supposition of these being the abscissæ of Parabolas.*

Hours.	Observed Temp.	Calculated.	Difference.
H. ′	°	°	°
8 27 P. M.	Mean, 48.266	Mean, 48.266	0.000
9	47.829	47.904	+0.075
10	47.276	47.315	+0.039
11	46.803	46.806	+0.003
12	46.398	46.374	—0.024
1 A. M.	46.134	46.021	—0.113
2	45.933	45.747	—0.186
3	45.689	45.551	—0.138
4	45.449	45.433	—0.016
5	Min. 45.394	Min. 45.394	0.000
6	45.653	45.555	—0.098
7	46.283	46.039	—0.244
8	47.029	46.845	—0.184
9	48.055	47.973	—0.082
9 13	Mean, 48.266	Mean, 48.266	0.000
10	49.012	49.091	+0.079
11	49.050	49.969	+0.019
12	50.777	50.653	—0.124
1 P. M.	51.149	51.141	—0.008
2	51.470	51.434	—0.036
3	Max. 51.532	Max. 51.532	0.000
4	51.239	51.422	+0.183
5	50.872	51.091	+0.219
6	50.294	50.544	+0.250
7	49.544	49.773	+0.229
8	48.624	48.783	+0.159
8 27	48.266	48.266	0.000

The numbers in column 3. were calculated by the following formulæ. From the property of the parabola, we have

$$BH : Bm = HA^2 : mn^2 \text{; and}$$

$$Bm = \frac{BH \times mn^2}{HA^2}.$$

But since AE is the line of mean temperature, pn the depression of the temperature below the mean at the point of time p, and $pn = Hm = HB - Bm$, then, calling μ the mean temperature, and y the ordinate mn, we have the required temperature T at the time p, thus:

$$T = \mu - HB + \frac{HB \times y^2}{HA^2};$$

or if m is the *minimum* temperature of the daily curve, then, at the point of time p, we have

$$T = m + \frac{HB \times y^2}{HA^2}.$$

For the *semi-parabola* BC, the formulæ are as follows:

$$T = \mu - HB + \frac{HB \times y^2}{CH^2}; \text{ or}$$

$$T = m + \frac{HB \times y^2}{CH^2}.$$

For the *semi-parabola* CD, in which the required temperatures *exceed* the mean, we have at any point of time p,

$$T = \mu + GD - \frac{GD \times y^2}{CG^2};$$

or calling M the *maximum* temperature of the daily curve,

$$T = M - \frac{GD \times y^2}{CG^2}$$

For the *semi-parabola* DE, we have the following formulæ:

$$T = \mu + GD - \frac{GD \times y^2}{EG^2}; \text{ or}$$

$$T = M - \frac{GD \times y^2}{EG^2}.$$

Upon comparing the parabolic abscissæ in column 3. with the observed results in col. 2., it appears, that the greatest difference

is *a quarter of a degree of Fahrenheit*, and that the differences are most perceptible in the afternoon branch of the curve, between 4 P. M. and 8 P. M.

We have no hesitation, however, in saying, that the mean of a greater number of years will produce a close approximation to the parabola. In 1824, the afternoon branch is irregular. In 1825, which was a year of uniform character, the afternoon branch becomes more convex, and approaches closely to the parabolic branch; so that the mean of 1824 and 1825 which we have given in col. 2. of the Table, and contrasted with the parabolic abscissæ, partakes of the irregularities of 1824, and thus occasions a flatness in the curve, and consequently the differences observed between 3^h P. M. and $8^h\ 27'$ P. M.

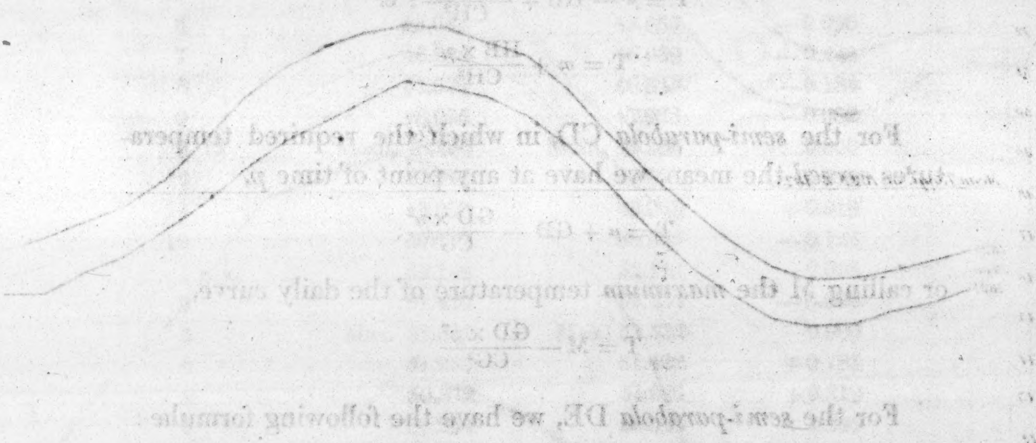
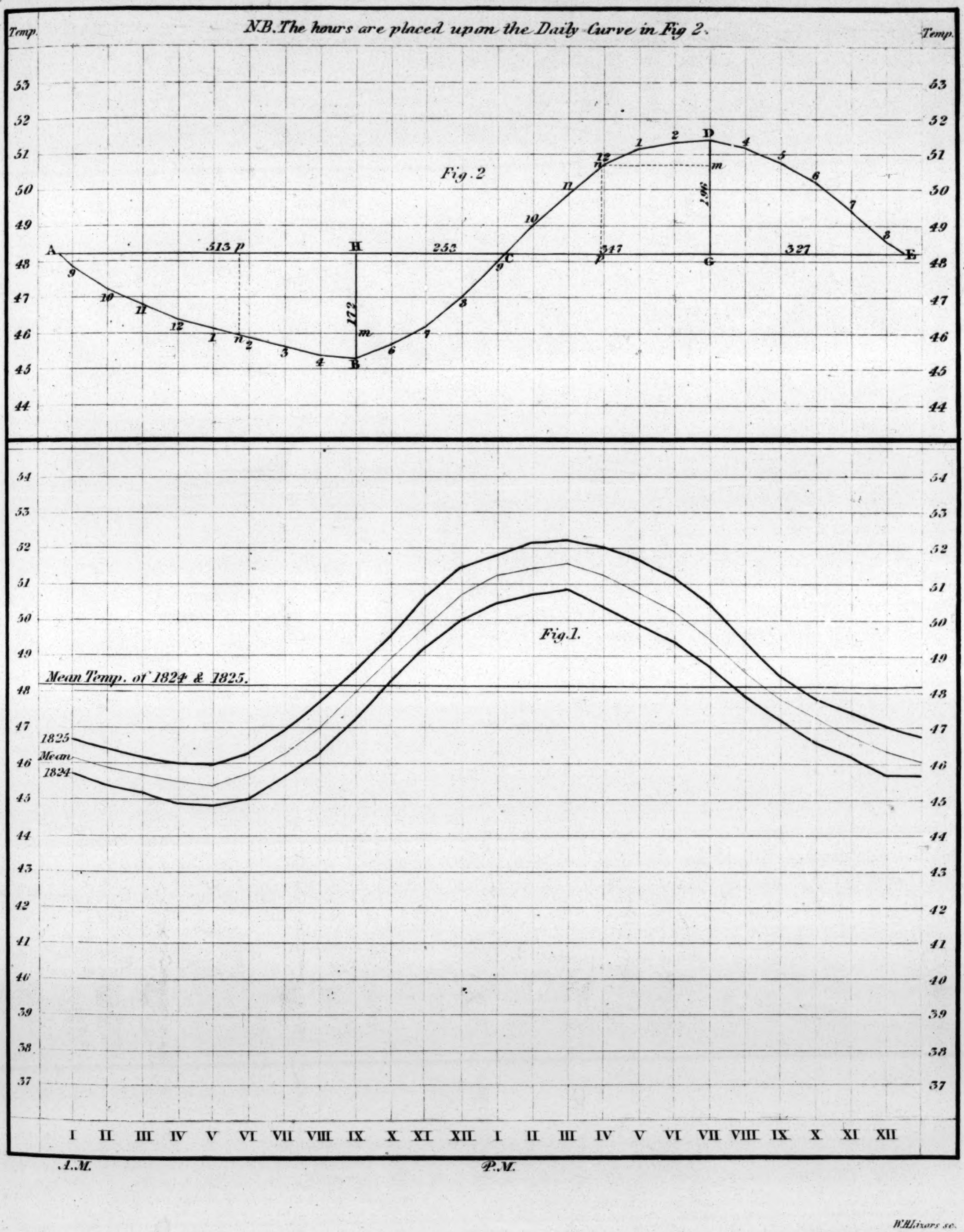


PLATE XIV.

Eng^d for the Royal Society Tran. Vol. X p 388

Mean Annual Curves of Daily Temperature in 1824 and 1825

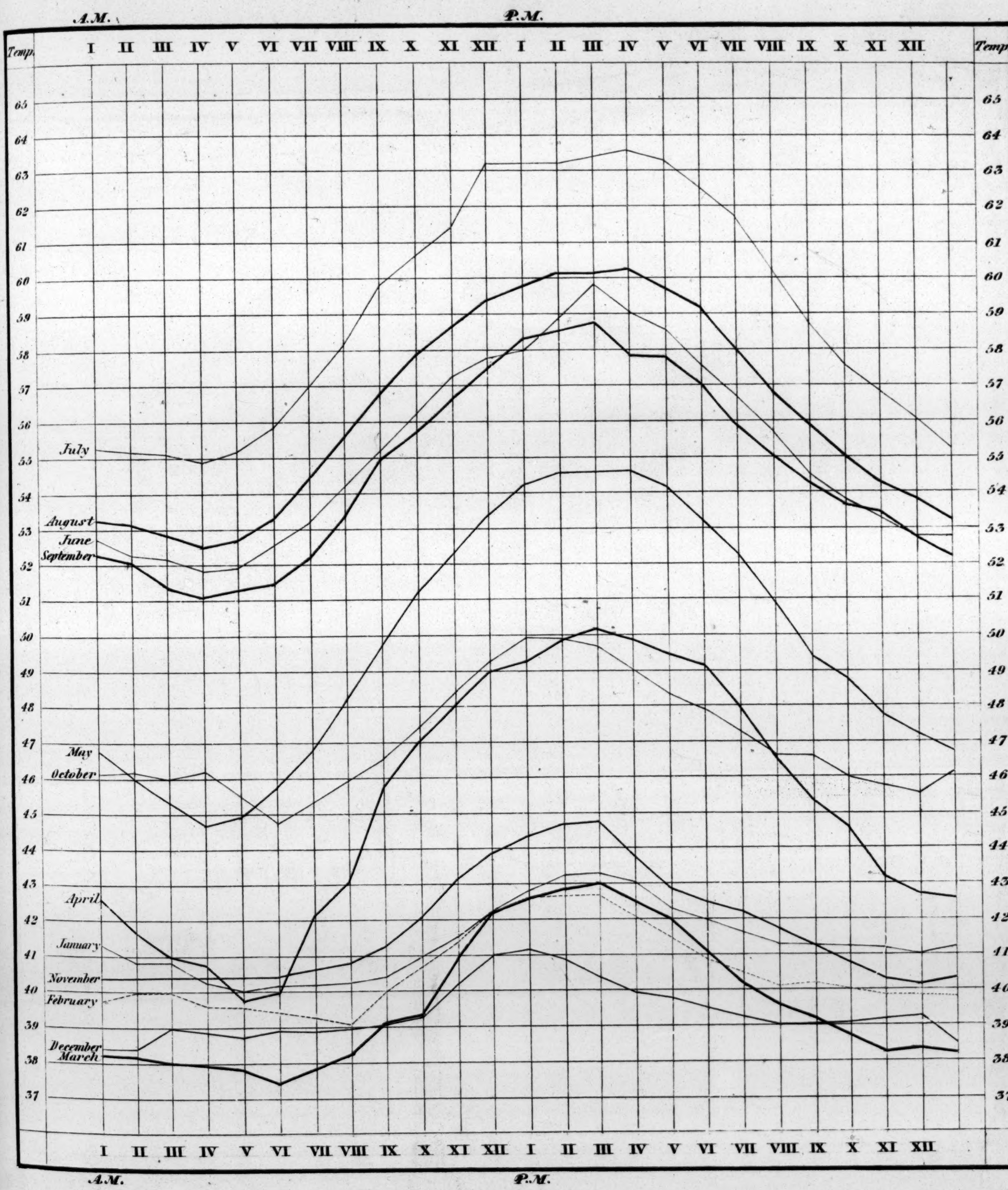


W.H.Lizars sc.

PLATE XV.

Eng.d for the Royal Society Tran. Vol. X p. 388

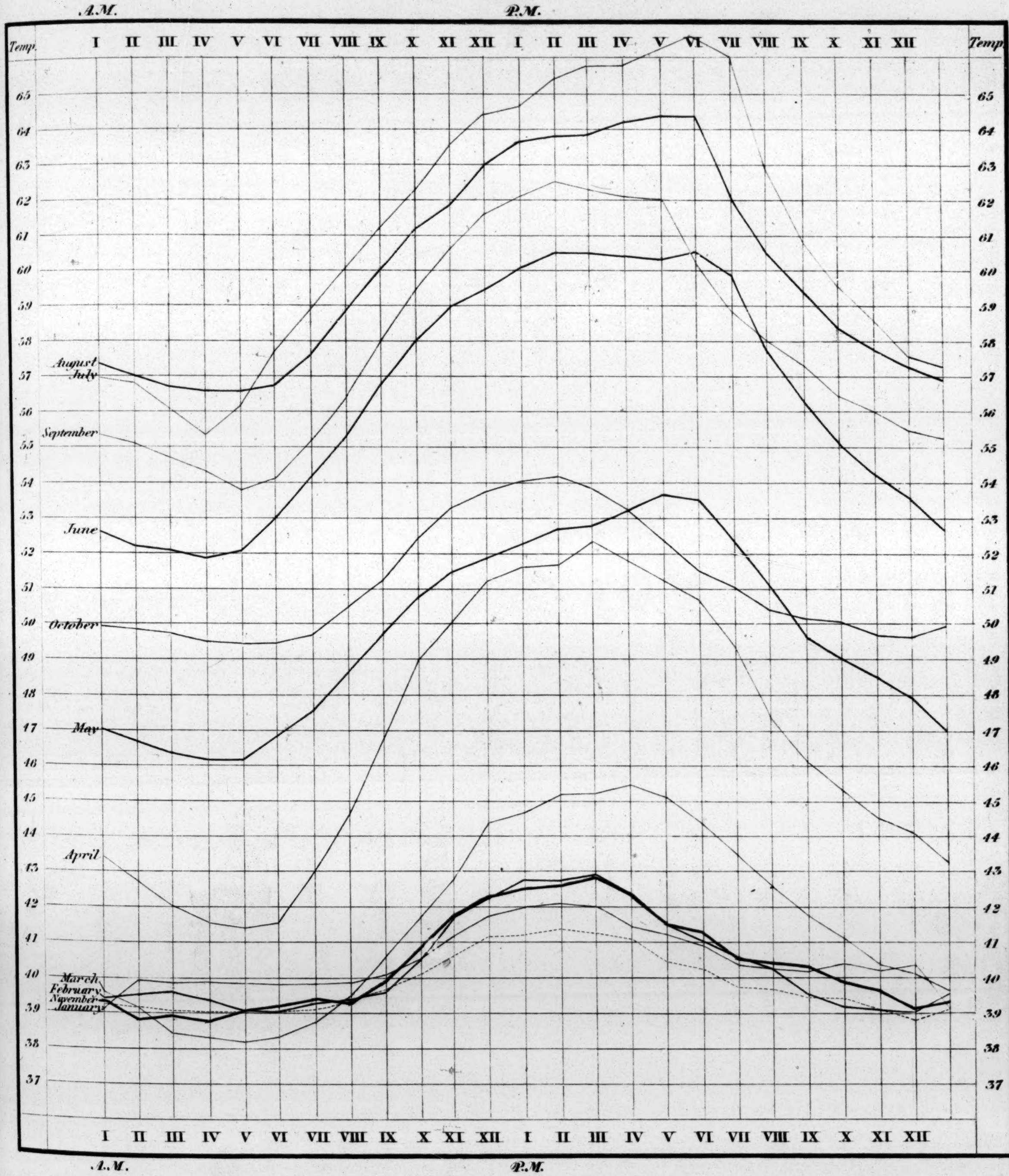
Mean Daily Curve for each Month of 1824



W.H.Lizars sc.

PLATE XVI. Eng.d for the Royal Society Tran. Vol. X p. 388

Mean Daily Curve for each Month of 1825

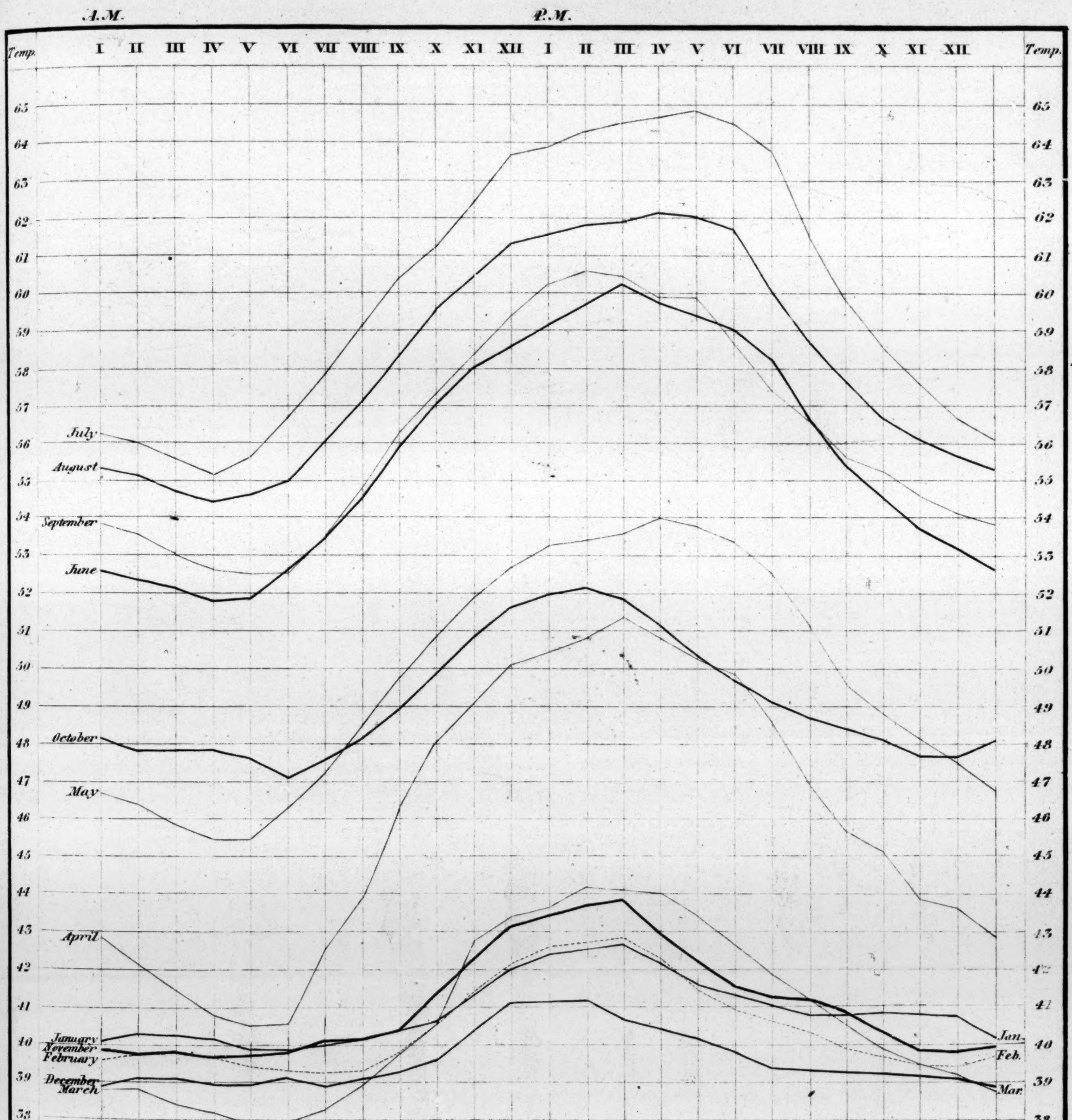


W.H. Lizars Sc.

PLATE XVII.

Eng.d for the Royal Society Tran. Vol. X p. 388

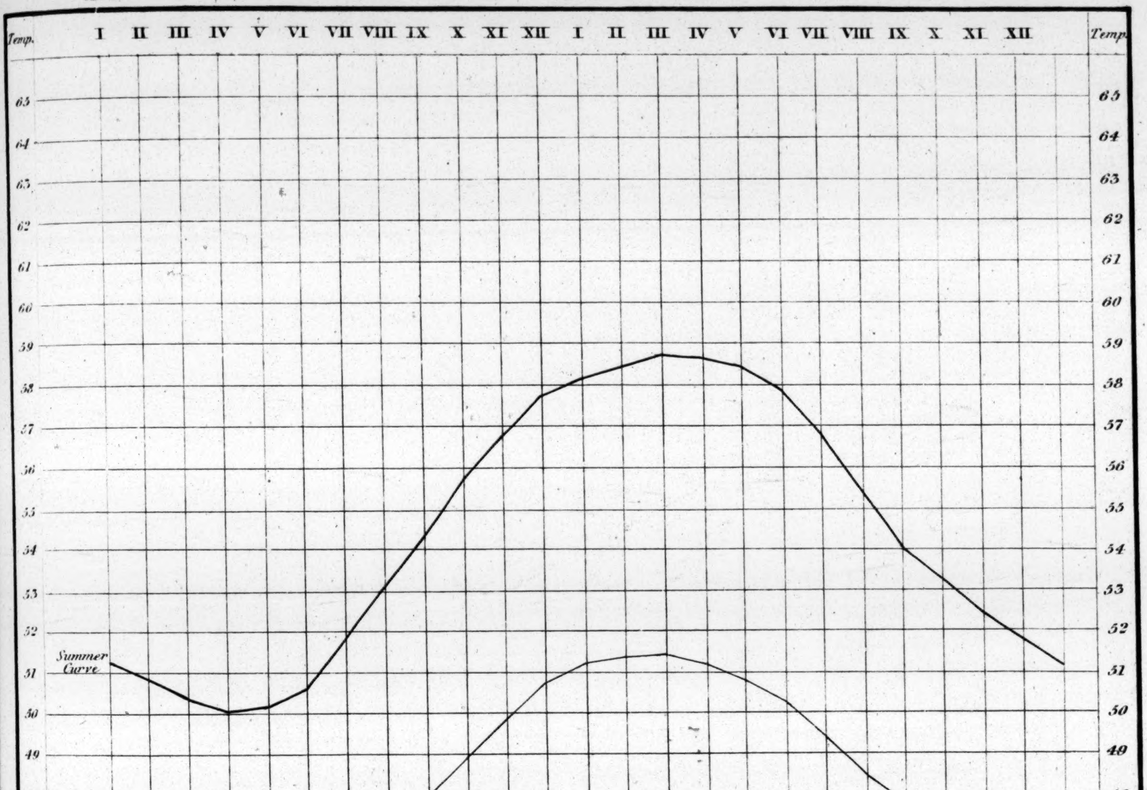
Mean Daily Curve for each Month of 1824 and 1825



W.H. Lizars sc.

PLATE XVIII. *Eng.d for the Royal Society Tran. Vol. X p. 388*

Mean Daily Curves of the six Summer & six Winter Months of 1824 and 1825



W.H. Lizars sc.

XXVIII. *A Historical and Critical Introduction to an Enquiry into the Revival of the Greek Literature in Italy, after the Dark Ages.* By PATRICK FRASER TYTLER, Esq. F. R. S. E. & Sec. Lit. Class.

(Read 21st Feb. 1826.)

A HISTORY of the revival of literature in Europe, after its extinction in the middle ages, has been long a desideratum in the annals of human knowledge; and from the wide, and almost untravelled field, which such a history would embrace, and the recondite sources of information which must be consulted, it will perhaps be long before any individual is found with sufficient learning to estimate its difficulties, and yet hardy enough to attempt to overcome them. Lord BACON * has presented us with an eloquent and comprehensive sketch of the subjects which ought to be included in a general history of learning. "The task is none other (says he) than that of extracting, from every possible source of information, a history of the sciences and the arts, and marking the different æras, and the various regions in which they have flourished. In this must be pointed out their earliest origin, their progress to perfection, their travels throughout different climes, and amid various tribes of people; for we know well that the sciences are migratory like nations. The historian must mark them in their declension, in their somnolency, in their revival. In his attention to individual arts, he must

* De Augmen. Scient. B. ii. c. ix.

explain not only their origin, but the causes to which they owe their invention, the manner in which they have been transmitted, the peculiar discipline, and the particular institutions under which they have been improved and perfected. But the task is not concluded. It is moreover the business of such a moral historian, to note the most celebrated sects and controversies which have sprung from the opinions of learned men, the persecutions they have suffered, the glories and the honours they have won. The most celebrated authors, the best books, the most learned academies, schools, endowments, associations of philosophic spirits, in short, every thing which regards the state and history of literature, must be carefully described." Such is a feeble translation from the energetic latinity of the original, presenting us with an outline, which, as has been remarked * by a later author, is the most perfect which a scholar could devise, but which no scholar can hope to complete. Dr Johnson, as we learn from his delightful biographer Boswell, " would have a history of the revival of learning contain an account of whatever contributed to the restoration of literature, such as controversies, printing, the destruction of the Greek empire, the encouragement of great men, with the lives of the most eminent patrons and professors of all kinds of learning in different countries." To the execution of this plan, which, although sufficiently vast, is but a corner of the mighty design of Bacon, the works of Tiraboschi, of Brucker, of Morhoff, of Baillet, and, latterly, of Guingené and Sismondi, present us with materials, both of a philosophical and of a critical nature, which are in the highest degree valuable. The original volumes of the great restorers themselves, the now

* Preface to " An Introduction to the Literary History of the 14th and 15th " Centuries," p. 5.; an anonymous but able and elegant work, published by Cadell and Davies, 1798.

neglected folios of Ficinus, of Erasmus, of Politian, of Budæus, of the Scaligers, of Casaubon, and their fellow-labourers in the same field, compose a mine in the present day almost unworked, yet full of the richest ore. But it is out of this very circumstance, the voluminousness and the value of the materials, that the embarrassments of the undertaking arise; and the mind which had courageously sketched out the plan of the edifice, may find, when it comes to struggle with the difficulties of execution, how wide is the difference between the reveries of literary ambition, and the realities of literary labour.

When we glance over the various branches of this great subject, there is perhaps no one division which is more interesting and important than the revival of the Greek language, and with it the Greek literature in Italy, during the latter part of the fourteenth century. For more than seven hundred years it had been almost wholly lost; and in the ages which elapsed between the fall of the Western Empire, in the close of the fifth, and the incursions of the Turks upon Europe in the fourteenth century, not only Italy, but Greece itself had been covered by successive swarms of barbarians. To trace the causes which led to the revival of the language and literature of Greece, after the dark ages, and to give some account of the lives and writings of the eminent scholars to whom we owe their restoration, is the object of the following historical enquiry; But, before immediately proceeding to this subject, there are two preliminary questions, upon which it will be necessary to give a short introductory disquisition. These are, I. What was the brightest period of the Grecian language and literature in its own soil, and to what extent does it appear to have been cultivated in Italy, after Greece had become a part of the Roman Empire? II. What were the effects produced by the inundation of the barbarous

nations; and what was the period of the total extinction of the Greek language and literature in Italy?

The Grecian language appears to have reached its highest perfection in that long and bright period, of almost six centuries, which extended from the days of Homer till the death of Alexander the Great. Hesiod, the great lyrists Tyrtæus, Alcæus, Sappho, Anacreon, and Pindar, the dramatic giants Æschylus, Euripides, and Sophocles, the historians Herodotus and Thucydides, the orators Lysias, Isocrates and Demosthenes, the fathers of philosophy Plato and Aristotle, and the historian, soldier, philosopher, and accomplished man of letters Xenophon,—all these high and gifted spirits, whose names and works have survived the calamities of more than two thousand years, were born, and wrote and died in this brilliant division of time.

This, says Harles, was the æra of the youth and manhood of the Greek language. "It was the æra of its poetry, which at first flourished in solitary excellence; the æra of its eloquence, which was created and encouraged by the constitution of the Athenian Government, by the manners of that remarkable people, their forms of judicial administration, the distribution of public honours, and the liberty of thought and discussion which was permitted in Athens. It was an æra full of talent in almost every branch of human knowledge, and fertile in minds of the most splendid genius,—in poets, orators, philosophers and historians,—and these all, or chiefly, belonging to that wonderful little republic of Athens *.'

The decay of Grecian literature is to be dated from the destruction of Grecian liberty. In the three succeeding centuries which intervened between the death of Alexander and the

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* Harles, Literaturæ Græcæ Notitia brevior, p. 25.

reign of AUGUSTUS, although the deterioration of the Grecian language was at first scarcely discernible, yet the seeds of change were but too surely sown. Greece itself was now occupied by the Romans, that new and mighty nation which had already acted so grand a part in the history of the world, and the Attic muses, as if attracted and dazzled by the Roman triumphs, deserted their native valleys, and fixed their seats in the beautiful fields of Italy. THEOCRITUS and MENANDER, the first in the sweetness of his pastorals, and the second in the playful elegance of dramatic poetry, shed, indeed, an auspicious ray over the commencement of this period. POLYBIUS, too, published his admirable Commentaries; and a crowd of philosophers, sophists, and rhetoricians, preserved, in their works of controversy and criticism, some shadowy traces of the perfection of this noble language. But, with metaphysical subtilties, there crept in new forms of expression. A mixture of foreign nations, and a familiarity with less perfect and polished tongues, polluted gradually its ancient purity; and, to use the words of HARLES, those symptoms of decay were visible, which told too surely that the freshness and vigour of manhood were past, and the infirmities of age approaching *.

If such was the state of Grecian literature and philosophy upon the death of AUGUSTUS, under whose reign the Roman muses were destined to enjoy their highest triumph, it will readily be believed that the three succeeding centuries, which filled up the interval from the accession of TIBERIUS to the reign of CONSTANTINE the Great †, brought only increasing decay and corruption to the pure and nervous language of DEMOSTHENES and XENOPHON. Works of talent, and even of genius, were not wanting. The geographical labours of STRABO ‡, PTOLEMY ‖,

* HARLES, p. 209.

† From A. D. 14 to 306.

‡ STRABO died A. D. 25.

‖ PTOLEMY, A. D. 161.

and Pausanias; the multifarious and amusing compilations of Plutarch *; in philosophy, the amiable and original meditations of the Emperor Antoninus †, are, of themselves, sufficient to vindicate this age from any imputations as to deficiency in enterprise, or imbecility in thought and reasoning. Nor ought the moral historian to forget, that the same era was illuminated by the brilliant satyric wit of Lucian ‡, and instructed by the criticism of Longinus ‖. Some few historians also, yet of inferior note, (Ælian, Dio Cassius, and Herodian), put forth their feeble efforts; but, with the single exception of the philosophic soldier Arrian §, nothing was produced in history in any degree worthy of the better days of Greece. If, however, even in the most favourable instances, we turn from the works themselves to the language in which they are written, the change is at once apparent and mortifying. The "undefiled well of Attic purity," the clear, strong, various, yet simple language of Greece, in its better days, had become insensibly but deeply polluted. The Roman conquests, the mixture of strangers, the incursions of the barbarians, the controversies between the Christians and the supporters of the ancient philosophy, were the chief causes which contributed to produce this deterioration; and if, at the conclusion of the second period, and in the days of Augustus, the approach of age was distinctly visible upon the fabric of this once noble language, in the days of Constantine these symptoms were not only confirmed, but increasing in strength, and hastening to their consummation. "Flos Atticæ elegantiæ atque eloquentiæ (says Harles) magis magisque deflorescere cœpit, et sensim periit ¶."

* Plutarch died A. D. 120.

† Antoninus, A. D. 161.

‡ Lucian, uncertain, probably in 170.

‖ Longinus, slain A. D. 275.

§ Arrian lived under Hadrian, A. D. 147.

¶ Harles, p. 303.

Such being a slight sketch of the revolutions of Grecian literature, from the age of HOMER till the era of CONSTANTINE, it will not be unimportant to consider for a few moments the second part of our first preliminary question, namely, To what extent does Grecian literature appear to have been cultivated in Italy after Greece itself was incorporated as a part of the Roman empire? And, in the *first* place, It is evident that the Roman people had no opportunities of becoming familiar with the poetry or literature of Greece during its first and most brilliant period. At the death of ALEXANDER the Great, the Romans were a brave but yet an infant people, or rather tribe, engaged in an obstinate struggle with the Samnites, obliged to defend their territories against the invasion of PYRRHUS, and, after the subjugation of both these rivals, embroiled in a most fierce and lengthened war with the Carthaginians. Immediately after the commencement of the third Punic War *, their legions, for the first time, entered Asia, and, with that overwhelming impetuosity which characterizes the progress of their arms at this period, overturned the ancient kingdom of Macedon, reduced the Achæans, and soon became masters of Greece. It is at this period of the third Punic War, that the earliest traces of a literary spirit are to be discerned in the history of the Republic,—that PLAUTUS, ENNIUS and TERENCE began to imitate the masterpieces of Greece. From this period of the dawn of Latin poetry, till the days of AUGUSTUS, the literature and the language of Greece, if not completely familiar to the nation in general, as some writers have erroneously supposed, were certainly well known to the poets, the orators, and the historians of Italy. It may even be asserted, that, down to a much later period in

* An. C. 149.

the history of the Western Empire, the literature of the Roman people, and the education of the Roman youth, evinced not, indeed, an acquaintance with the purest Grecian writers, but at least a knowledge of the language and philosophy of Greece, such as they then existed, polluted indeed, and obscured, but still retaining distinct traces of their original brightness. The proofs of this assertion are certain and multifarious. The few noble fragments which remain to us of Ennius, the father of the Latin epic school, his study of the great Grecian models; the well-known popularity of Menander; the translations of many parts of Xenophon, Plato and Demosthenes, by Cicero; the fact that this great Orator could declaim in Greek *; the assertions of Suetonius concerning the profound knowledge of this language, which had been attained by Claudius; its cultivation under some of the succeeding Emperors; and the injunctions of Quinctilian, who recommends that his youthful pupils should be introduced to an acquaintance with this noble language, even prior to the study of their own; all these facts very clearly show, that the study of Greek letters was pursued not only by the Poets and Orators, who there sought for their highest models of imitation, but that it formed at Rome an important branch in the education of its youth.

If such was the universality of the knowledge of the Grecian language and literature in the days of Augustus and his successors, an acquaintance with these great sources of beauty and wisdom became still more prevalent in the reigns of Hadrian, Antoninus Pius and Marcus Aurelius. In the commencement of the second century, Hadrian was an enthusiastic and unwearied patron of the Greeks and their language. The temples of Athens were rebuilt by his munificence, the public games re-

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* Palæoromaica, p. 26, 33.

stored, and a noble library, and new gymnasium, provided for the instruction and exercise of youth. Education, says CHANDLER, now flourished in all its branches at Athens. The Roman world resorted to the schools, and reputation and riches awaited the able professor. At this period Athens abounded in philosophers. It swarmed, according to LUCIAN, with cloaks, and staves and satchels; you beheld every where a long beard, a book in the left hand, and the walks full of companies discoursing and reasoning. The enthusiasm of HADRIAN was seconded by the efforts of his successors ANTONINUS and MARCUS AURELIUS, both of them philosophic Emperors, and both deeply smit with the love of Grecian literature. After the death of AURELIUS, for more than a hundred years of crime and bloodshed, the Greek language presents in its fate and fortunes a striking contrast to the more melancholy destinies of its sister the Latin. It was preserved, certainly not in its original purity, yet in a state far removed from decay, in the works of LUCIAN, DIO CASSIUS, HERODIAN and LONGINUS. CONSTANTINE the Great, although himself little of a Grecian scholar, yet by the removal of the seat of empire from Rome to Constantinople, promoted the more general study of this language. It is well known, that to the Emperor JULIAN, even from the days of his earliest youth, the religion A. D. 363
and philosophy of Greece were subjects of peculiar predilection, and that this extraordinary man considered the Greek language as his native tongue, and the language of Rome as a foreign and less familiar dialect *.

Such is a slight sketch of the fate and fortune of Greek literature in Italy, down to the momentous period when the northern nations, by partial inroads upon the frontiers, began to threaten the empire, which they finally destroyed.

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* Palæoromaica, p. 40.

Towards the middle of the third century, some indications of an irruption of the northern nations were discernible; but these formidable enemies may be said to have contained themselves within their original settlements, until, in the fourth century, an inundation of the Huns drove the northern tribes from the countries where they had hitherto led a warlike and migratory life, and compelled them to seek permission of the Emperor Valens to settle in Thrace. This was granted, probably it could not have been prudently denied, and the historians of these times affirm, that the whole of Thrace was almost instantly covered by successive waves of this living flood of men. Macedonia and Pannonia soon became entirely occupied by the multitudes of stranger emigrants; their tents were even pitched upon the classic borders of Thessaly; and war, as was to be expected from the proximity of such formidable neighbours, was almost instantly commenced with the Roman Empire. At the head of his Goths Alaric penetrated to the borders of Italy, and the pusillanimity of the Emperor Arcadius was content to purchase an ignominious peace, by ceding to him the whole of Greece. His next object was the conquest of Italy, which, after his army had been reinforced by a new inundation of the Suevi, Alani and Vandals, he concluded by the sack of Rome, in the commencement of the fifth century. It is well known that he died when meditating the conquest of Sicily and Africa. Every successive year now more effectually confirmed the dominion of the northern nations over the wide extent of the Western and Eastern Empire.

Rome, as we see, was taken in the beginning of the fifth century. In the same period the Vandals had established their monarchy in Spain. Carthage soon after fell, and the Roman provinces in Africa became subject to the same victorious people. Attila, the scourge of the human race, next appeared, to act his terrible part in the extermination of ancient nations; and, lead-

ing his armies of Huns, overran and desolated the Roman provinces of Dacia, Thrace, Mysia, and Syria. It would be tedious and unnecessary to enumerate all the triumphs of the north, and all the defeats of the once illustrious Romans. Rome herself, the mistress of the world, was successively taken by GENSERIC king of the Vandals, ODOACER chief of the Heruli, and THEODORIC prince of the Ostro-Goths, who, from this period acknowledged by the eastern Emperor ZENO as king of the country which he had subdued, commenced an able and glorious reign. The brave and unfortunate BELISARIUS retrieved, for a while, by his victories, the ebbing honours of the Western Empire, and TOTILA the Goth, who, in the middle of the sixth century, had again ravaged Italy, and twice become master of Rome, was defeated and put to death by the eunuch NARSES. But these temporary triumphs only paved the way for the final and conclusive victories of the Lombards, another swarm from the northern hive, who, towards the conclusion of the sixth century, invited into Italy by NARSES, to revenge his individual injuries, succeeded in reducing to subjection the greatest part of the country. Their empire continued to flourish from the end of the sixth till the middle of the eighth century, when it was finally overturned by CHARLEMAGNE, who, in the city of Pavia, was crowned King of the Lombards, in the year 744.—Let us pause for a few moments, to consider the effects of these barbarian inundations upon the literature of that great country in which they took place.

The conquests of ALARIC, in the fifth century, did not materially affect Italy. It was evacuated by the barbarians, and, although governed by ADOLPHUS, a relation of ALARIC, it was governed by Roman laws, and the institutions and manners of the Roman people remained unchanged till the second conquest of the country by ODOACER, the first barbarian king of the West. Yet although the Roman laws and language remained, the race

of this ancient people was even at this period fast approaching to extinction. Immense multitudes of Romans, who had formerly enjoyed dignity and fortune, were reduced to the state of captives in the barbarian armies, or slaves who cultivated their lands. Multitudes were driven, and multitudes voluntarily retired, into exile, consenting to drag on a dependent existence in the remotest provinces of the empire. Want and famine, which, in the exhausted state of the provinces, were not unfrequent visitants, carried off the victims which had been spared by the ravages of war, many of those who remained, intermarried with the barbarian families, and all these co-operating circumstances began at this time to have a strong effect in producing that physical and moral degradation of this once illustrious people, which, in the course of the succeeding century, and shortly after the settlement of the Lombard princes, concluded in the total disappearance of the Roman race. For although still the names of Roman families remained, nothing but the name was left. All else was "second childishness and mere oblivion."

Theodosius the Great * succeeded to the empire in the latter part of the fourth century; and, during his reign, although the highest qualities of a soldier were successfully and brilliantly exerted, yet the accumulated difficulties which on every side threatened the Western Empire, left little leisure for the princely patronage of literature, or the peaceful acquisitions of knowledge. The death of Theodosius sealed the fate of the Roman Empire, which, after lingering through the feeble reigns of his unremembered successors, closed its mighty history of twelve centuries, in the sack of Rome by Odoacer king of the Heruli †.

The Herulian dominion in Italy, which lasted only for seventeen years, was concluded, and the empire of the Ostrogoths

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* A. D. 379. † A. D. 476.

established, by the taking of Ravenna at the termination of the fifth century. THEODORIC, although of barbarian extraction, had been educated at the Court of Constantinople, and not only himself, but his secretary CASSIODORUS, and his minister BOETIUS, were familiarly acquainted with the language and literature both of Rome and of Greece, the cultivation of which appears to have been a very general passion amongst the learned statesmen who surrounded his Court. But Grecian literature, as we have already seen, had, at this period, fallen into a state of melancholy weakness, even on its own soil, and the beautiful language of Attica was now no longer what it once had been.

In those dark ages, when beneath the loss of civil liberty, the decay of ancient nations, and the inundation of barbarian hordes, the cause of knowledge and of science was too speedily losing ground; the noble stand which was made for it by the Fathers of the Christian Church ought not to be forgotten. Infinitely superior to their pagan opponents, in the ardour with which they devoted themselves to literary pursuits, and philosophical studies; the sublimity of their doctrines, and the excellence of their moral precepts, acquired additional strength from the classical purity of their style. In the department of Grecian literature, a lustre is thrown over the third century by the single name of ORIGEN, the theologian *, the philosopher, the grammarian, the adamantine pillar of the Church. Among the Latin Fathers, and in the same century, names of no common eminence are to be found. The dialogue of MINUCIUS FELIX †, a Roman lawyer and Christian convert, is remarkable for the elegance of its Latinity, and for the interesting picture which it presents to us of

* ORIGEN was born at Alexandria in the year 186.; HARLES, p. 644.; SPANHEIM, p. 248.; CAVE, Historia Literaria Eccles. p. 112. vol. i.

† CAVE, Hist. Literaria, p. 101. vol. i.

the manners of the early Christians; and there are few to whom the history of their religion is a subject of interest, who have not heard of Cyprian Bishop of Carthage, of his unwearied labours, his unshaken piety, his eloquence, his misfortunes, and his martyrdom *.

If the cause of letters was so deeply indebted to the Christian Fathers in the third century, the exertions of the same enthusiastic and learned scholars become still more brilliant during the fourth and fifth centuries, when contrasted with the increasing darkness of paganism. Gregory Nanziansene †, whose mind, although engrossed in his labours as a Christian orator, had imbibed in the schools of Athens the love of the ancient philosophy; and Chrysostom, whose studious and abstemious youth, nursed for six years in the solitude of the desart ‡, ripened into a manhood of unremitting toil, and almost unrivalled eloquence; these two great men were sufficient of themselves to oppose a very successful barrier against the inroads of ignorance and barbarism. The compositions of Chrysostom are celebrated not only by contemporary critics, but by the fastidious philologists of the sixteenth century, as admirable for their Attic purity of style ‖; and, in the West, the writings of Lactantius, Hilary of Poitiers, Jerome and Augustine, were serviceable not only in their zealous, though sometimes ill directed endeavours for the protection of the infant Church from heresy, but in the preservation of the purity of the Latin language.

I may briefly advert to two remaining causes, which at this period had a powerful effect in preserving from total extermination the relics of the learning of Greece and Rome; and one of

* Cyprian was slain in the year 258. Cave, Hist. Literar. p. 126. vol. i.

† Cave, Hist. Literar. p. 246. Gregory flourished in the year 370.

‡ Cave, Histor. Literar. p. 300.

‖ Chrysostom was born in 354.

which was destined to be eminently effectual in the restoration of letters. The first of these is to be found in the writings of the civilians; the second in the rise of monastic establishments in the fourth century. SALVUS JULIANUS, by order of the Emperor ADRIAN (says that learned author formerly quoted) framed the perpetual edict, or a standing code to extract the essence of preceding institutes, and exhibit an authentic body of salutary laws. His successors distinguished themselves by industry and learning. Proficients in philology, from the necessity of a close application to the most ancient writers, they employed their knowledge to correct and refine their language. Well versed in the fashionable philosophy of Greece, they did not amuse themselves with the investigation of metaphysical subtleties, or the involution of moral precepts, but devoted their acquirements to define the rights, and protect the property, of their fellow-citizens. It need not be insisted how much such a body of writers have done for the cause of learning, in counteracting the earlier affectation, and the later barbarisms of cotemporary authors. Even when the day of destruction came, they still furnished the most essential services. It was the diffusion of their writings over the provinces, and the use of the Roman jurisprudence in legal "decisions, that served to preserve the memory, and almost to embalm the purity of the Latin tongue *."

In this passage the important effects ascribed to the pleadings and the writings of the Roman lawyers, throughout the period of four hundred years, which elapsed from the reign of ADRIAN till that of JUSTINIAN, in the beginning of the sixth century, are not overrated; and the three Emperors, to whose encouragement we mainly owe this preservation of the Latin language, are ADRIAN, THEODOSIUS and JUSTINIAN.

* Introduction to the Literary History of the Fourteenth and Fifteenth Centuries, p. 23, 24.

The obligations which the cause of letters owes to monastic establishments, were different in their nature, but equal in their importance. During the persecutions of the Christians in the fourth century, which at this period were carried forward with unremitting barbarity, a convert to the new religion, to escape the terrors of death or torture, concealed himself in the desart of Egypt. His life of abstinence and solitary piety caused many devout persons to repair to the desart; and, during the continuance of the persecution, the love of life, combined, with the ardour of devotion, to increase the numbers who flocked to the cave of this holy man; and, either in emulation of his austerity, or perhaps under the idea of imitating the Divine Author of their religion, betook themselves to prayer and seclusion in the caves and mountains *.

The passion for the monastic life increased in an almost incalculable degree during the succeeding centuries. The different orders established by these recluses, soon spread their ramifications not only throughout the East, but over the greatest part of Europe, and, fortunately for the interests of human knowledge, an eager love of learning, such as it then existed, induced the monks to found libraries, to establish schools, to transcribe ancient manuscripts, and to preserve at least, if they could not appretiate, the invaluable volumes of antiquity.

But, upon the conquest of Italy by the Lombards, a darker and more melancholy spectacle succeeds. Their dominion in Italy continued for two centuries, and, under their iron yoke, the literature both of Greece and of Rome became entirely extinguished. Hitherto the barbarian tribes had respected the conquered people. The Gothic race under Theodoric had become amalgamated with the Roman. The luxury and enervating influence of Italian wealth, of the manners and the climate of this

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* Spanheim, Epitom. ad Hist. Nov. Testam. p. 273.

voluptuous country, had tamed the pride, and softened the barbarity, of the northern invaders. But the Lombards were in every respect a different people. They broke into Italy from their native settlements in Pannonia, while all the vigour and savage freedom of barbarism was yet fresh and unworn upon them, and in their conquest of that kingdom, they treated the degenerate Romans with cruelty and contempt.

" This ferocious nation," says St Gregory, " is come upon us " like a tempest, and, thundering on our defenceless heads, has " ravaged our cities, levelled our fortresses, destroyed our mona- " steries, and almost exterminated the inhabitants. Our fertile " fields have no longer cultivators or proprietors, and places once " populous are occupied and defiled by beasts of prey *."

Greece was now the only country where the light of science and of literature still remained unextinguished, and where the knowledge of the works of antiquity was still cultivated with enthusiasm; but Italy could not profit by this circumstance; for, to fill up the cup of her misery, an almost perpetual war subsisted between the Lombards and the Greeks, and all hopes of a second dawn from this wonderful country, in whose literary history there seems to have been no middle age of darkness, were thus completely extinguished.

The works of the philosophic Boetius, composed about the beginning of the sixth century, independent of their own intrinsic beauty, acquire thus a reflected interest from the gloomy period in which they were written, and, in their perusal, we truly seem to listen to the latest sighs of the expiring literature of Greece and Rome.

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* St Gregory in his Exposition of the Prophet Ezekiel. See Baronius *apud* Howels, vol. 3. The Translation is by the author of the Dissertation.

This moral eclipse of all that was excellent in human knowledge, continued for nearly six centuries, during which period the country of Virgil successively presents to the eye of the historian a confused assemblage of different races of men, Franks, Normans and Saracens; and when at length, after a long period of war and bloodshed, the light of civilization breaks in upon the scene, we find that out of this living chaos, there had arisen the infant nations, and the unformed language of modern Italy.

The irruption of the Lombards into Italy, is the gloomy period from which we may date the total extinction of the Greek language and literature in the West *; nor do we find any distinct traces of a spirit of revival until the middle of the fourteenth century, the age of Petrarch and Bocaccio.

* Baldelli Vita di Bocaccio, p. 223.

XXIX. *On the Refractive Power of the Two New Fluids in Minerals, with Additional Observations on the Nature and Properties of these Substances.* By David Brewster, LL.D. F.R.S. Lond., Sec. R.S. Edin., and Corresponding Member of the Academy of Sciences of Paris.

(Read March 6. 1826.*)*

In the Paper which I had the honour of submitting to the Society *, on the Two New Fluids in mineral bodies, I have given the index of refraction for the most expansible of the two, as it exists in the cavities of *Amethyst ;* but as I had not then ascertained the refractive power of the second fluid, and as the principal phenomena of the two fluids, especially those which related to their properties when taken out of the cavities, were observed in specimens of Topaz, it became desirable to have an approximate measure of the refractive power of both of them, as they exist in that mineral. As the fluid in Amethyst had never been examined in the open air, its identity with that in Topaz was inferred solely from the equality of their expansion by heat, so that the determination of the refractive power of the latter was necessary to establish either a difference between these two substances, or their perfect identity.

In the repetition of the experiments described in that paper, and in extending my inquiries to different specimens of Topaz, I sought diligently for a cavity whose shape and situation in the crystal would enable me to obtain an accurate measure of the refractive power of the two fluids. Such a specimen I have had

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* See Page 1. of this Volume.

the good fortune to meet with; and one of the principal objects of the present paper, is to give an account of the results which it enabled me to obtain.

To those who are acquainted with the doctrines of refraction, it is scarcely necessary to state, that if m is the index of refraction of any substance, such as Topaz, the sine of the angle at which light incident on the second surface of it will suffer total reflexion, will be $\frac{1}{m}$, and if any fluid is in contact with that surface, the sine of the angle of total reflexion will be $\frac{m'}{m}$, the index of refraction of the fluid being m'. Hence

$$m' = m \times \text{Sin. Angle of Total Reflexion};$$

so that the index of refraction of the fluid is easily deducible from the angle of total reflexion.

When the surface of the cavity is parallel to a face of cleavage in the plate of Topaz which contains it, the angle of total reflexion cannot be observed without cementing a prism upon one of these faces; but as this tended to make the experiment more complicated, I sought for a cavity, the faces of which were inclined to the two parallel faces obtained by cleavage. This cavity, shewn in Plate XIX. Fig. 1., consisted of a vacuity V;—of a large portion NN of the highly expansible fluid,—and of a considerable quantity MM of the second fluid, which suffered almost no change by heat. The situation of this cavity in the specimen is shewn in Fig. 2., where C is a section of the cavity perpendicular to its length MM, Fig. 1., and inclined to the parallel cleavage planes EF, GH of the Topaz.

In a room where the temperature was about 60° of Fahrenheit, I fixed this specimen upon a goniometer, and I measured the angle of incidence at the surface EF, when the light of a candle RD, incident on the vacuity, began to suffer total re-

flexion. This angle was 38° 42′. From the index of the ordinary refraction of Topaz, which is 1.620, I computed the angle of refraction CDB to be 22° 42′ and the angle of total reflexion DCP to be 37° 38′ 35″. Hence the angle ADC was 67° 18′; the angle ACD 52° 21′, and DAC, the inclination of the face of the cavity to the refracting surface EF, was therefore 60° 21′.

Calling x the inclination of AB to EF, or DAC and φ the angle of refraction CDB, we shall have $x = \angle$ total reflexion $+ \varphi$. For in the similar triangles ADB, CPB right angled at D and C, we have CAD = CPB. But CPB = DPQ = CDB + DCP, that is $x = \angle$ Total Reflexion $+ \varphi$.

The goniometer remaining steady in its place, the divided circle and the crystal were turned round, till the same ray RD began to suffer total reflexion from the refracting surface of the expansible fluid NN Fig. 1. and the Topaz; and the new angle of incidence KDR′, at which this took place, was found to be 26° 39′. The goniometer being turned still farther, the same ray suffered total reflexion, from the separating surface of the second fluid MM and the Topaz, when the angle of incidence KD was 11° 52′.

These results obviously enable us to determine with accuracy the refractive power of the Two New Fluids.

Calling θ the angle of incidence, φ, φ' the angles of refraction, m m', m'' the indices of refraction for Topaz, the expansive fluid and the second fluid; then we have $\sin \varphi = \frac{\sin \theta}{m}$; $\varphi' - x =$ Angle of Total Reflexion, and

$$m' = m \times \text{Sin}\,(\varphi' - x)$$
$$m'' = m \times \text{Sin}\,(\varphi'' - x)$$

Hence we have the indices of refraction as follows:

$m = 1.620$ Topaz,
$m'' = 1.2946$ Second Fluid.
$m' = 1.1311$ Expansible Fluid.

The following Table will shew the relations of the indices of refraction of these two new substances to those of other bodies which I have found to possess a refractive power lower than Water.

TABLE of Refractive Powers lower than Water.

Water,...	1.3358
Cyanogen * rendered fluid by pressure,.........	1.316
Ice, ...	1.3085 †
Second New Fluid in Topaz, in a cavity which is filled by the other new fluid, at the temperature of 83°....................................	1.2946
New Fluid in Amethyst, which fills the cavity at a temperature of 83¾° of Fahrenheit,......	1.2106
Tabasheer, whitish, from Nagpore, hard specimen, ...	1.1825
Tabasheer, transparent, from Nagpore,...........	1.1503
Do. do. another specimen, ...	1.1454
New Expansible Fluid in Topaz, in the same cavity as the second fluid, whose index of refraction is given above	1.1311
Transparent Tabasheer from Vellore, of a yellowish tint, ...	1.1111
Ether expanded into nearly thrice its original bulk, ...	1.057

I have not made any attempt to measure the refractive power of the new expansible fluid, after it has filled the cavity, having satisfied myself with observing, that the angle of total reflexion diminished when the fluid was in this expanded state ‡. In

* This Cyanogen was made by Dr TURNER. Mr FARADAY, who first rendered it fluid, remarks "that its refractive power is rather less, perhaps, than that of water." *Phil. Trans.* 1819, p. 286.

† This is a mean between Dr WOLLASTON's result and mine.

‡ This experiment is a very interesting one to the spectator; the new fluid, appearing quite transparent at a temperature of 60°, seems quite opaque when it is made to fill the tube, by a slight increase of temperature, as if it had become black by heat.

those cavities where the vacuities are so large that the fluid is converted into vapour before it fills them, the refractive power of it, as measured when the cavities are full, will obviously be much less than that which is obtained when the substance retains its fluidity, and it will vary in different specimens, and even in different cavities of the same specimen, according to the proportions which the vacuity bears to the quantity of the expansible fluid.

Additional Observations on the New Fluids in Minerals.

Having had occasion to shew the various phenomena of the new fluids to several distinguished foreigners, and to others who took an interest in the subject, I have thus been led to continue my examination of minerals in relation to these remarkable substances. Some of my scientific friends, who were anxious to repeat these experiments, have experienced great difficulty in obtaining specimens of minerals containing the cavities of fluid. This difficulty has no doubt arisen, from their examining the well crystallised specimens which are generally found in the cabinets of mineralogists. If they had broken up with the hammer only a few of the rounded or imperfectly crystallised white topazes from Brazil or New Holland, they could scarcely have failed to discover, with the compound microscope, innumerable cavities fitted for the purposes of observation. After a little practice in splitting and preparing the specimens, in which, from the perfection of the cleavage planes, the aid of the lapidary is almost never required, the patient observer will experience no difficulty in detecting cavities of every variety of form, and in discovering the fluid as it flows from the opened cavities over the planes of cleavage. Mr SANDERSON, lapidary in Edinburgh, who takes a great interest in every pursuit of a scientific nature, has succeeded in obtaining some of the finest specimens of these new fluids; and by cutting and polishing the Topazes which contain

them, so as to exhibit the cavities to the best advantage, he has been enabled to shew most of the phenomena to those who are interested in such pursuits. Several of these specimens are of great value, as will appear from the drawings and descriptions of some of the most remarkable, which will be given in the sequel of this paper.

In the additional observations which I have now to submit to the Society on this subject, I shall confine myself to the following heads:

1. On the Number and Arrangement of the Fluid Cavities.
2. On the Form of the Cavities containing the New Fluids.
3. On the condition of the Fluids within the Cavities.
4. On the condition of the Fluids when taken out of the Cavities; and
5. On some miscellaneous phenomena connected with the formation of Fluid Cavities.

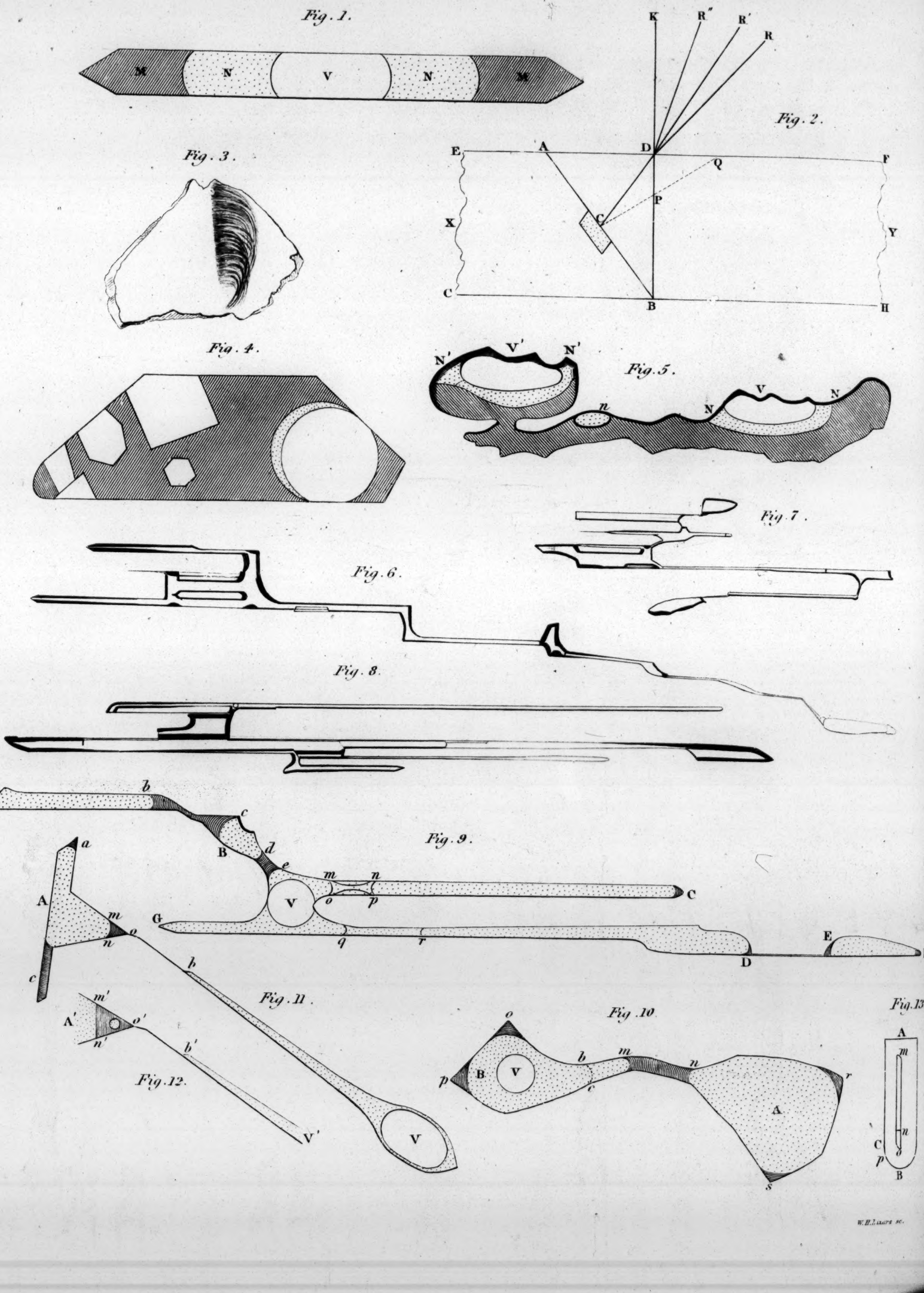
1. *On the Number and Arrangement of the Fluid Cavities.*

In a former paper I had occasion to mention, that, in a specimen of Cymophane about $\frac{1}{7}$th of an inch square, I counted 30,000 cavities. Although this statement occasioned great surprise, and some expressions of scepticism, yet it was too feeble to convey any idea of their number. So minute are these cavities, that the highest magnifying powers are often necessary to render them visible; and we might as well attempt to number the grains of sand on the sea-shore, as to count these fluid cavities when they appear in this minute state.

The strata in which these cavities are arranged, are not so closely related to the primary and secondary planes of the crystals as I formerly supposed. I have found them in almost every

PLATE XIX.

Engd. for the Royal Society Tran. Vol X. p. 413.



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possible direction, and intersecting one another at angles which cannot be referred to any of the crystalline forms of the mineral. In a specimen of Quartz observed by Mr Somerville, and now in the possession of Mr Sivright, they are arranged in hollow groupes somewhat like the cells of a honeycomb; and when they are viewed by reflected light, the corresponding faces of the cavities are seen to be parallel, though the cavities have every possible variety of position with respect to each other. In other specimens they form planes of variable curvature, and sometimes curved surfaces of contrary flexure; and in one specimen belonging to Mr Sivright the longitudinal cavities are grouped and inflected, so as to resemble a curled lock of the finest hair, as shewn in Plate XIX. Fig. 3. In a specimen of Blue Topaz from Brazil, belonging to Mr Spaden, lapidary in Edinburgh, there are no fewer than four strata of cavities nearly parallel to each other, and in the thickness of one-eighth of an inch. The cavities have a different character in each stratum, and their number is such as almost to destroy the transparency of the plate.

In the distribution of most of these groupes, accident seems to have had the principal share; but there are certain modes of distribution that appear to be the result of some general principle; and a more diligent examination of them, as well as of others which may yet be discovered, will probably throw farther light upon the origin of this class of phenomena. In a specimen, for example, belonging to Mr Sanderson, and shewn in Plate XXI. thirteen times its natural size, an immense number of cavities are arranged in rectilineal groupes, radiating from a centre A. Each rectilineal group consists of *two*, or in some places *three*, rows of cavities, and several of the radiations are bent from their original direction. The spaces between each pair of rows are filled with curiously branching cavities, some of which are half an inch long; but the remarkable fact is, that these cavities are connected with numerous slender branches, many of which

communicate with a single cavity in the nearest rectilineal row of the radiations between which the long cavities are placed.

They have a resemblance to lakes or rivers, whose branches have been supplied from these rows of cavities; though it is more likely that the expansion of the fluid within the long cavities, and when the substance of the topaz was soft, forced out a great number of globules, some of which continued to adhere to the slender filamentous cavities from which they were discharged.

In all the cavities of this remarkable specimen capable of being examined, there are found both the new fluids, with the exception of the long branching cavity AB, from which they had escaped, in consequence of the end A being cut by the lapidary. The dense fluid always occupies the filamentous branches.

In some cases there is a breach of continuity in the branches, a small part of the cavity being as it were filled up with solid topaz. This fact favours very much the supposition that all the rows of minute cavities had been thrown off from the great ones; though the rows of cavities on the left and lower side of the specimen are hostile to it.

The plane in which these cavities lie is perfectly flat, and is nearly perpendicular to the axis of the prism, the line joining the two resultant axes of double refraction being parallel to MN.

2. *On the Form of the Cavities containing the New Fluids.*

In a former paper I have given drawings and descriptions of some of the most remarkable shapes which these cavities assume; but in the prosecution of the subject, I have met with a variety of new and remarkable forms. In a specimen belonging to Mr Sanderson, and which is one of the most valuable that I have seen, each cavity (see Plate XIX. Fig. 6, 7, and 8), consists of

Engd for the Edinr Royal Society Tran. Vol X. p. 415 p. 4

Fig. 1.

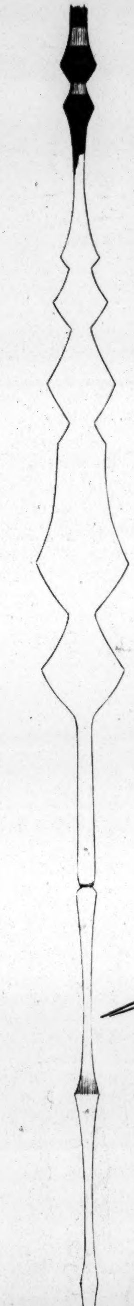


Fig. 2.

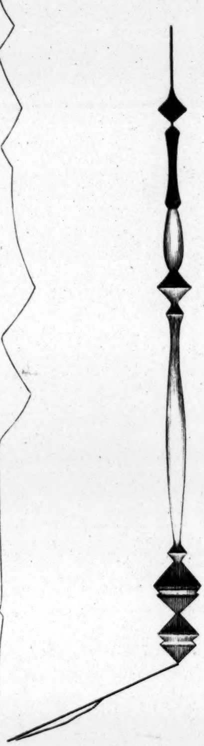


Fig. 3

Fig. 4.

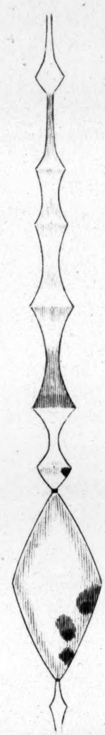


Fig. 5.

Fig. 6.

Fig. 9.

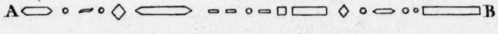
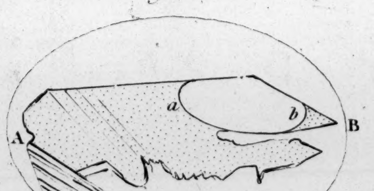


Fig. 10.

Fig. 7.

Fig. 8.



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a variety of cavities of different lengths and sizes, bounded by parallel lines, and communicating by narrow channels, which almost escape the cognisance of the microscope. In these cavities thus curiously combined, the two new fluids are arranged in the most remarkable manner, the dense fluid occupying all the necks, and angles, and narrow channels, while the expansible one is left in the open and less capillary spaces. When the heat of the hand is applied to the specimen, the fluids in the cavities are all set in motion. The dense fluid quits its corners, and assumes new localities; and the different portions of the expansible fluid either unite into one, or are subdivided by the interposition of some portion of the dense fluid, which has been expelled from its primitive situation, and drawn into its new position by capillary action. When the specimen is allowed to cool, the two fluids quit their new position; and, as if they were endowed with vitality, they invariably resume the same positions which they occupied at the commencement of the experiment.

Another form of the cavities still more remarkable occurs in a very fine specimen belonging to Mr Sivright. These cavities resemble a number of parallel cylinders, as shewn at AB in Fig. 7. Plate XX; but, owing to some cause which it is difficult to conjecture, a number of them have been afterwards turned aside towards C, so as to be open at one of their extremities. From these extremities, which terminate in the surface ACB, the fluids have made their escape, and have left the interior of the cavities lined with a black and brown powdery residue, which always remains after their evaporation. When the cavities thus inflected and deprived of their fluids are submitted to the microscope, they exhibit the most extraordinary shapes; some of which are represented in Figs. 1, 2, 3, 4, 5, and 6, of Plate XX. They have the appearance of having been formed by a turning

lathe; and such is the symmetry and beauty of their outline, that it is not easy to conceive that they are the result of any mechanical cause. One of these cavities, which is unconnected with the rest, resembles a finely ornamented sceptre, as shewn in Fig. 2., in which the proportion and forms of the different parts are executed in the finest taste. But what is most remarkable, the different parts of this figure lie in different planes, so that, when it is seen in a direction at right angles to that of symmetry, it appears merely a number of disjointed lines, as in Fig. 10.

The inflexion of the cavities AB into the directions *b*C, &c. and the discharge of their fluid contents at the surface ACB, could only have taken place when the whole mass ACB, though crystallised, had not attained its permanent induration *. This opinion derives great support from the fact, that the lines *b*C are perpendicular to the axes of the prism, and consequently lie in the planes of most eminent cleavage. The direction, therefore, in which the fluids were discharged, was *that of least resistance*,—a result which might have been expected.

In the specimen now under consideration, there is a stratum of fluid cavities, composed of a great number of parallel rows of cavities, and remarkable for their symmetry. One of these rows is somewhat like AB, Fig. 9. If we now suppose that when this specimen had not acquired its permanent state of induration, the fluids in its cavities were expanded by a considerable heat, the fluid in one cavity would force itself into the adjacent ones, so that the row of cavities AB would form one cavity, somewhat like that in Fig. 6. If the cavities lay in different planes, as shewn in Fig. 10., then the expanded fluid would descend to the

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* Patrin, if we recollect rightly, speaks of crystals of Beryl in Siberia, which were so soft that they broke like a piece of apple.

one immediately below it, and connect the whole together as in Fig. 2. We do not mean to say, that the cavities *b*C in Fig. 7. were actually formed in this manner, because this is rendered improbable by their connection with the rectilineal ones AB, but merely to explain how cavities having the forms shewn in Figs. 1—7. may have their origin from the union of a great number of cavities arranged as in Fig. 9.

When the cavities are regularly crystallised, which is frequently the case in quartz and topaz, the homologous sides of the hollow crystals are parallel to one another, and also to those of the primitive or secondary form which they resemble. In some very curious but amorphous specimens of quartz from Brazil, belonging to Mr Spaden, the hollow crystals terminate in six-sided pyramids, *with flat summits*, and the axes of these pyramids is parallel to the axis of the system of polarised rings, and consequently to the axis of the crystal.

3. *On the Condition of the Fluids within the Cavities.*

The phenomena of the expansible fluid have been so fully described in my former paper, that I have only a few observations to add upon this part of the subject. In some specimens of quartz, the expansible fluid seems to exert a very considerable elastic force, even at the ordinary temperature of the atmosphere, and when a very small heat is applied, it sometimes has sufficient force to burst the specimen. A very remarkable case of this kind happened to a son of Mr Sanderson, who put one of the Quebec crystals of quartz into his mouth. Even with this small accession of heat the specimen burst with great force, and cut his mouth. The fluid which was discharged had a very disagreeable taste.

The extreme volubility of the expansible fluid, and its power of penetrating even the hard topaz in which it is inclosed, were

exhibited in a very remarkable manner, which I have described and attempted to delineate in my former paper. Upon applying heat to a specimen of quartz, the elastic force of the imprisoned fluid was such as to make it force its way through the solid stone; and when it had made its escape into the open air, not a trace of its path was left behind. This phenomenon, which was too extraordinary to present itself frequently, was afterwards seen both by Mr Sanderson and myself in a specimen of topaz, when the fluid ascended through its substance with great rapidity, and resembled globules of quicksilver. This metallic appearance was owing to the total reflexion of light, which took place at the refracting surface of the globule and the topaz. That the fluid in this case forced its way through the cleavage planes of the mineral cannot be doubted, and I have in another paper shewn, that fissures in glass may be closed up without leaving the slightest trace of the two surfaces ever having been separated *.

In the various cavities described in my former paper, the whole of the expansible fluid, when exposed to heat, was either driven into vapour †, or retained its fluidity after it had filled the vacuity. Since that paper was published, however, I have discovered cavities in which, after the application of heat, there may be said to be *three different substances*, viz. 1. The expansible fluid in a state of fluidity; 2. The dense fluid; and, 3. The vapour of the expansible fluid. This curious fact will be understood from Fig 5. of Plate XIX, which represents a cavity in a specimen belonging to Mr Spaden. The cavity is *one-twelfth* of an inch long. The expansible fluid is lodged at N N and N′ N′,

* See *Phil. Trans.* 1816, p. 73.

† One of the largest vapour cavities that I have seen is *one-twelfth* of an inch every way. It is less than half full of fluid, and hence it is driven into vapour by heat. During the precipitation of the vapour it becomes perfectly opaque.

where there are large vacuities V, V′, and there is a globular portion of it at *n*, without a vacuity. When heat is applied, the fluid at N N and N′ N′ quickly goes off into vapour; the portion at *n* expands into an elliptical globule, but its force is not sufficient to displace the mass of the second fluid between *n* and N, and *n* and N′; and being kept in equilibrio by the opposite and nearly equal expansive forces of the vapour in N N, and N′ N′, it consequently remains fluid at *n* *. In a plate of Topaz shewn to me by Mr Sivright, where the expansible fluid consists of two portions floating in a large quantity of the dense fluid, one of the portions is a spherical drop which expands with heat, and contracts with cold, exhibiting by transmitted light an effect similar to the opening and shutting of the pupil.

In re-examining the phenomena of the second or denser fluid, several very curious facts have come under my notice.

I had previously shewn, that, when several cavities communicated with each other, the narrow necks, or lines which joined them, were filled with the dense fluid, which shifted its position when the equilibrium of the adjacent portions was destroyed by heat; but I have since had occasion to examine the phenomena of the second fluid with more attention. The particles of this fluid have a very powerful attraction for themselves, like those of water, and they are also powerfully attracted by the mineral which contains it. The particles of the expansible fluid have, on the contrary, a very slight attraction for one another, and also for the mineral which incloses them. Hence it follows, that, as the two fluids never in the slightest degree mix with one another, the dense fluid is either attracted to the angles of angular

* In Fig. 4. of Plate XIX, I have represented another vapour cavity, which is remarkable for having a very small portion of the expansible fluid, and also, for having several crystalline forms within the dense fluid.

cavities, or occupies the bottom of round ones, or fills the narrow necks or channels by which two or more cavities communicate with one another. The expansible fluid, on the other hand, occupies all the wide parts of the cavities, and in those which are deep and round it lies above the dense fluid.

If we now apply heat to a single deep cavity containing both fluids, the elastic force exerted by the expansible one, after its vacuity is filled up, will modify the form of the dense fluid, pressing it out of some corners and into others, till the elastic force of the one is in equilibrium with the capillary attraction of the other.

But if there are two cavities, A, B, communicating with each other, as in Fig. 10. Plate XIX., where the dotted part represents the expansible fluid, then the dense fluid will be found in the neck at *m*, *n*, and at the angles *o*, *p*, *r*, *s*. Let us now suppose that there is a vacuity V only in the smaller cavity B, and that heat is applied to the specimen. It is obvious that the greater expansion of the dotted fluid in A, which has no vacuity to fill, will force the dense fluid *m n* towards V, where it will take up a new position about *b m c* when the expansive forces are *in æquilibrio*. But if the cavity A is very large compared with B, the fluid *m n* will be driven out of the neck *b n*, and will find its way to some of the corners *o*, or *p*, from which, upon cooling, it will again return to its position *m n*.

Let us now suppose that the cavity A communicates with other cavities which expand slowly into it, while it is expanding into B; then, at every expansion of A, the dense fluid *m n* will be driven to a side, but it will immediately return, opening and shutting like a valve. This effect is finely exhibited in an irregular branching cavity of a specimen belonging to Mr Sanderson; but as the expansions and contractions are too numerous and complicated, I shall describe them as existing in a cavity of a more simple structure, represented in Fig. 9. Plate XIX, by A B

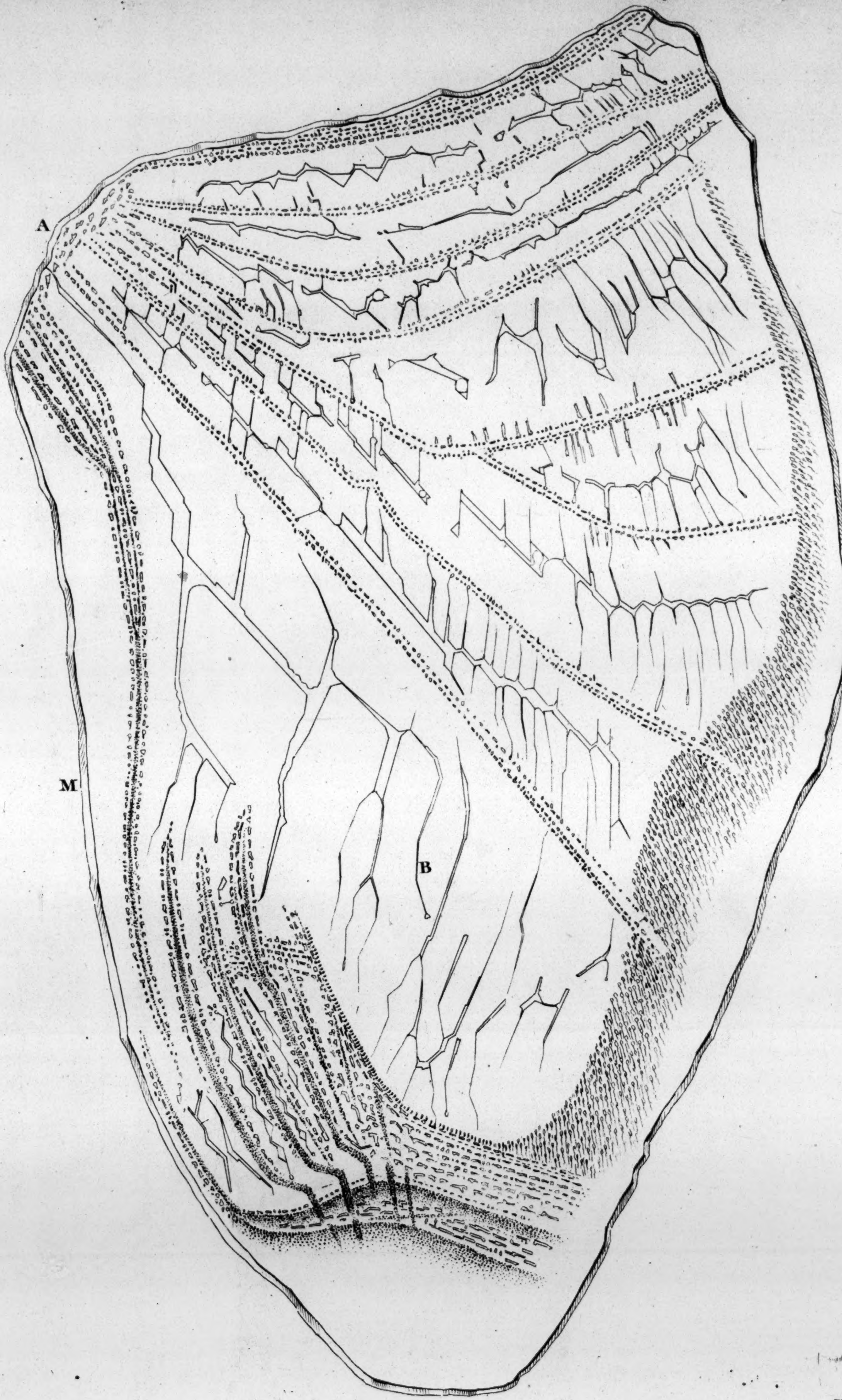
C D E. In ordinary temperatures, about 45°, there is a vacuity of the size V, in the expansible or dotted fluid, and the dense, or shaded fluid, occupies the necks *b c*, *d e*, DE, and also the extremity F. By applying the heat of the hand to the specimen, the expanding fluid in the branches V C, V D, finds space for itself, by filling up the vacuity, but as there are no vacuities in the portions of expanding fluid at A B, B, and E F, they must necessarily force out the dense fluid which confines them. The dense fluid in the neck E D, is thus made to appear at D, and the whole of the dense fluid at *b c* is driven off to *d e*, till, accumulating there, it is drawn by attraction to the nearest neck, *m n o p*. Here it first lines the circumference of the hollow neck, from its powerful attraction for topaz; and, as the lining becomes thicker, it appears as a slight elevation between *o* and *p*, and between *m* and *n*. These elevations increase till they leap together by their mutual attraction, and form a column of the dense fluid *m n p o*. The column *b c* of dense fluid has now disappeared entirely, and the space A B C D is filled with the expanding fluid. The heat of the hand being continued, the expanding fluid A B forces itself through the little cylinder of dense fluid *d e*, which resumes its place the moment that a portion of the former has passed. But as the same heat has been expanding the fluid between *n p* and C, which pushes out part of the dense fluid at *m n o p*. this dense fluid, and the surplus of what was displaced from *b c*, moves along the sides of the cavity till it occupies the portion *q r*, of the branch V D. Sometimes the dense fluid is entirely driven from *m n o p*, and part of it sent to the extremity C; though, in general, a very small portion remains at the very neck *m o*.

As the specimen cools, the dense fluid quits *m o* and *q r*, and is gradually transferred through the neck *d e* to the neck *b c*; every portion of it invariably resuming the very position which it had before the application of heat.

A very curious modification of these actions is seen in a cavity of the specimen shewn in Plate XXI., which I have represented separately in Fig. 11. of Plate XIX. The branch *b* V has always, at common temperatures, a vacuity V, and the cavity A, connected with it by the filamentous channel *o b*, has no vacuity. At the ordinary temperature, the dense fluid appears at *a c*, and slightly at *o* and *b*, filling the narrow channel *o b*. By applying heat, the expanding fluid in *b* V fills up the vacuity V; and, as the cavity A *a o c* has no vacuity, a portion of its fluid is necessarily driven through the neck *a b* into *b* V in small globules; but, owing to the narrowness of the neck at *b*, the phenomena are not easily observed. Upon cooling, however, the retransference of the fluid that had passed from A to *b* V, is finely seen. The contraction of the expanding fluid in A causes the dense fluid to appear as at *m n o*, in Fig. 10., and, in a short time, the curved surface *m n* becomes more flat; and, at last, a straight line, as at *m′ n′*, Fig. 12. This indicates a pressure along the canal *b′ o′*, in the direction *b′ o′*, and a bubble of the expansible fluid instantly issues from *o′*, as in Fig. 12., and, passing through the dense fluid, joins the expansible fluid in A′. After three or four of these have passed, the equilibrium is restored. In this case, the capillary force exerted by the channel *o′ b′* upon the dense fluid which it contains is too strong to permit the little globule of the expansible fluid in *b′* V to displace it, as in Fig. 9., so that it passes very slowly in separate globules.

The *fluid valves*, as they may with propriety be called, which thus separate the different branches of cavities, afford ground of curious speculation in reference to the functions of animal and vegetable bodies. In the larger organisations of ordinary animals, where gravity must in general overpower, or at least modify, the influence of capillary attraction, such a mechanism is neither necessary nor appropriate; but, in the lesser functions of the same animals, and in almost all the microscopic

Engd. for the Edinr. Royal Society Tran Vol. X p. 422.



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structures of the lower world, where the force of gravity is entirely subjected to the more powerful energy of capillary forces, it is extremely probable that the mechanism of immiscible fluids, and fluid valves, is generally adopted. We must leave it, however, to the physiologist to determine the truth of this supposition.

In the second, or dense fluid, whose motions we have now described, there exist frequently black spicular crystals, which may be made to move to different parts of the cavity. Whether or not these crystals are extraneous bodies, or indicate the commencement of that induration of the fluids which I have described in a former paper, is a point which can only be ascertained by observing the progressive changes which the crystals may undergo.

4. *On the Condition of the Fluids when taken out of the Cavities.*

I have already described so fully in a former paper the singular movements into which the expansible fluid is thrown, when it first flows out of its cavity upon the surface of the plate of topaz which contains it, that I have nothing to add upon this subject *. It did not then occur to me that these movements might be owing to electricity, till I read an account of the following experiment made both by Professor Erman and Mr Herschel. When a globule of water, dropped on the surface of a flat dish of mercury, is brought into connexion with the positive pole of a galvanic battery, while the mercury is connected with the negative pole, it instantly flattens and spreads to twice its diameter, regaining its former sphericity when the circuit is broken. This extension and subsequent re-aggregation of the globule of water,

* Some of the fluids in quartz seem to be entirely gaseous, while in sulphate of barytes the fluid appears to be the mineral itself in a fluid state; see p. 425. and note on p. 427.

is precisely the same effect as that exhibited by the drop of expansible fluid; and it is therefore very likely that the latter is owing to an electrical cause. In separating the particles of bodies, electricity is always produced; and in the cleavage of Topaz and Mica, even electric light is developed. But experiments are still wanting to determine, whether, in the present case, the electricity is derived from the separation of the cleavage planes, or from the change of condition which the new fluid is undergoing during its rapid evaporation, and its partial conversion into a powdery residue.

5. *On some Miscellaneous Phenomena connected with the Formation of Fluid Cavities.*

In my former paper, I have described the phenomena of a single fluid in the cavities of various minerals and artificial crystals *. Since that paper was written, I have seen many specimens of this kind; but as the fluid has always, when examined, been found to be water, such specimens possess no peculiar interest, unless their cavities are opened, in the manner first adopted by Sir Humphry Davy. One of these specimens, however, which was kindly sent to me for examination by W. C. Trevelyan, Esq. is so peculiar as to deserve notice. In the drawing of it, in Plate XX. Fig. 8., which is of the real size, A B is a cavity in quartz, which is filled with a fluid, excepting the vacuity *a b*, which may be made to move to different parts of the cavity. The

* Mr William Nicol, Lecturer on Natural Philosophy and Chemistry, has shewn me some fine specimens of Amber containing cavities. The inner surface of these cavities is rough, like finely ground glass, and many of them contain a fluid with a moveable globule of air. In a specimen of calcareous spar, in the possession of Mr Sanderson, there is a fluid cavity about *two inches long*, an inch wide, and one-eighth of an inch deep.

fluid does not expand perceptibly by heat, and is in all probability water. When the specimen is shaken, the fluid becomes turbid, and of a whitish colour, arising from a fine white sediment, which settles in the lower part of the cavity.

In a specimen of Quartz from Brazil belonging to Mr SPADEN, there is a cavity with an air-bubble, about the tenth of an inch long. It is nearly one-third full of a white powder, consisting of crystalline particles, which, upon inverting the specimen, flow over the surface of the air-bubble like sand in a sand-glass. In the specimens of quartz already mentioned in page 417. as containing cavities with pyramidal summits, there is only one fluid, in which there is generally an air-bubble. These cavities often contain opaque spherical balls *, which are distinctly moveable; and in one cavity I have counted *ten* of these balls, *seven* of which roll about the cavity when the specimen is turned round †. In a second specimen, spherical balls of the same kind are copiously disseminated in the quartz, and exist also in the cavities. In a third specimen, the balls occur near the summits of the pyramidal cavities, some of them being within and some of them without the cavity.

In the crystallisations of ice several phenomena occur, which are intimately connected with the preceding inquiry. When water is frozen in a glass vessel, the ice is often intersected with strata of cavities, which have the same general form and aspect

* These balls are of the same size as the seeds of *Lycopodium*, which amount to 32 parts of Dr YOUNG's eriometrical scale. Their diameter is therefore $\frac{1}{30000} \div 32 = \frac{1}{937}$th part of an inch.

† I have since opened several of these cavities by the blow of a hammer. In a second or two the fluid was entirely gone, without leaving a trace of its existence behind. The spherical balls remained in the cavities. They were not acted upon either by the muriatic or the sulphuric acids.

as those in minerals. I have sometimes observed frozen drops of dew, containing a portion of water which *remained unfrozen even at low temperatures;* and I have recently had occasion to examine some crystallisations of ice, which presented the same fact, under more curious circumstances.

A very sharp frost occurred in Roxburghshire on the morning of the 8th October 1825. The gravel-walks in the garden were raised up about an inch above their natural level by the sudden congelation of the water in the earth mixed with the gravel. All the elevated portions consisted of vertical prismatic crystals of ice of six-sided prisms, with summits which seemed to be triedral. The leaves of plants, &c. were covered with granular crystals, which were in general six-sided tables.

Upon examining with a microscope the prismatic crystals aggregated in parallel directions, they presented some curious phenomena. They had numerous cavities of the most minute kind, arranged in rows parallel to the axis of the crystals, and at such equal distances as to resemble a series of mathematically equidistant points. Some of the cavities were very long and flat, and sometimes they were amorphous; but in general they contained *water* and *air*.

Upon submitting one of these cavities to a powerful microscope, it appeared as shewn in Fig. 13. of Plate XIX, where A B C is the piece of ice, having in it a long cavity *m o*, containing water and air. The ice gradually dissolved; and when the end *n o* of the cavity *m n* was near the edge of the ice C B, the air, in a portion of it *n o*, detached itself, and went off at *p*, through the solid ice, the cavity closing up again at *n*. This phenomenon is analogous to the passage of the expansible fluid through topaz and quartz, which has been already described; the air in the one case, and the fluid in the other, finding its way in the direction of easiest cleavage, and the fissure closing up again in the manner already mentioned in a preceding part of this

paper. The singular fact, however, is, that the portion *n o* of the cavity quitted by the globule of air, was immediately filled up with ice, and the cavity reduced to the dimensions *m n*.

As the formation of ice from water is in every respect analogous to the formation of crystals from a substance rendered fluid by heat, the examination of its cavities is likely to throw some light upon their formation in mineral bodies *.

In concluding these observations, I could have wished to enter into some details respecting their geological relations; but as these would lead us too far into the regions of speculation I shall not enter upon them on the present occasion. It may be proper, however, to state, that the opinion which I hazarded in a former paper, that the discovery of the two New Fluids in minerals attached a new difficulty to the aqueous hypothesis, has been rendered more probable by every subsequent inquiry; and that I can see no way of accounting for the phenomena, but by supposing that the cavities were formed by highly elastic substances, when the mineral itself had been either in a state of fusion, or rendered soft by heat.

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* Since this Paper was written, Mr William Nicol has shewn me a very remarkable specimen of *Sulphate of Barytes*, with fluid cavities of the same general character with those which I described in my former paper (Trans. vol. x. p. 36.), but much larger than any which I had seen. Upon grinding down on a dry stone, one of the faces of this specimen, the largest cavity burst, and discharged its fluid contents through the fissure upon the ground surface of the specimen. The fluid lay in drops of different sizes along the line of the fissure, and in this condition Mr Nicol put it into his cabinet. Upon looking at the specimen about *twenty-four* hours afterwards, *each drop of fluid had become a crystal of Sulphate of Barytes.* These crystals had the primitive form of the mineral.

This very curious fact is analogous to the uncrystallised water in the ice-cavities mentioned above, the crystallisation in both cases being prevented by pressure. When that pressure was removed, a portion of the water and the fluid sulphate of barytes were immediately crystallised. Mr Nicol distinctly remarked, that the crystals occupied as much space as the drops of the fluid; so that the crystals of sulphate of barytes were not deposited from an aqueous solution, but bore the same relation to the fluid from which they were formed, as ice does to water.

XXX. *Observations on Two Species of Pholas, found on the Sea-coast in the neighbourhood of Edinburgh.* By JOHN STARK, Esq. M. W. S. Communicated by DR BREWSTER.

(Read 20th March 1826.*)*

THE Natural History of the Pholades, so far as regards their mode of burrowing in wood and stone, seems yet to be but imperfectly understood, though the Pholas was known to the ancients, and PLINY notices its phosphorescent quality *. RONDELETIUS †, JOHNSTON, and RUMPHIUS have figured several species; LISTER, among others, gives representations of three British species, the *Pholas dactylus, candida,* and *crispata* ‡; and Sir ROBERT SIBBALD, in his Prodromus, has three rude figures of the *dactylus* or *crispata,* as Scottish shells. None of these authors, however, attempted to explain how the Pholades exca-

* "His natura in tenebris, remoto lumine, alio fulgore clarere, et quanto magis humorem habeant, ludere in ore mandentium, lucere in manibus, atque etiam in solo et veste decidentibus guttis."—PLIN. lib. ix. c. 61.

† "Hæ in saxis latet, ut saxo undique contegatur, per foramen duntaxat exiguum et sensui vix patens aqua nutritus. Testis constat duabus longis, non in latum extensis mytulorum modo, sed rotundis. Intus eadem fere est caro quæ in mytulis." —ROND. *de Testaceis,* lib. i. p. 49. I strongly suspect, that RONDELETIUS has figured the *Mytilus lithophagus* under the title of Pholas; and that subsequent writers have been misled from not having seen his figures. The species of Pholas which he delineates is given under the name of *Concha altera longa.*—*Vide* ROND. p. 23, 27.

‡ "Hæ conchæ juxta Hartlepool frequenter reperiuntur, et in lapidis cujusdam cretacei foraminibus latitant ab ipso eorum ortu; nam ex his eximi non possunt, nisi prius lapis frangatur."—LISTER, *Anim. Ang.* p. 172.

vated their habitations in the rock, or perforated the submerged wood in which they seek protection. Bonanni, so far as I know, was the first who turned his attention particularly to this inquiry. In his work, entitled "Recreatio Mentis et Oculi," the first edition of which, in the Italian language, was published at Rome in 1681, he has given figures of the *Pholas dactylus*, and of pieces of the rock in which it was contained, shewing, with considerable accuracy, the nature of the perforations, and distinctly marking the circular lines at the base of the cells. These perforations are formed, in his opinion, by the action of the file-like valves on the stone, the animal fixing itself, for this purpose, by its callous foot to procure the necessary motion of its shell *.

The celebrated M. de Reaumur next took up the subject, without, however, seeming to have been aware of the prior investigations of Bonanni, whose book is neither quoted nor alluded to by the French naturalist. In the "Mémoires de l'Academie Royale des Sciences" for 1710, this intelligent observer has a paper on the progressive movement of some species of Bivalves; and in the volume for 1712 he gives the sequel of his observations on this curious subject. In this second memoir, after detailing the manner in which the Solenes burrow in the sand, he is led to consider the means by which the Pholas perforates the softer rocks; and this, he endeavours to prove, is done merely by the action of its muscular foot. The hardness of the substance perforated, however, induces M. de Reaumur to form a theory to account for an instrument, so apparently unsuitable, being able to perform what he ascribes to its action. The clay rock from the coast of Poitou and Aunis, on which his observations seem to have been made, was too hard on the surface to admit, in his mind, the supposition of its being bored by such

* Bonanni, *Recreat.* p. 36.

an implement; and he therefore concludes, that the Pholades must have entered the clay when it was in a soft state, and that it had been subsequently hardened or petrified by some viscous quality of the waters of the sea *. This theory, it may be remarked, leaves no room for the multiplication of the species; for, on the supposition that the clay has been hardened on the surface by some petrifying quality of the water, after the Pholades had made their lodgment, the same cause would operate to prevent the future races from commencing their cells †.

D'Argenville, with the knowledge, it appears, of what Bonanni and Reaumur had written upon the subject before him,

* In opposition to this theory, it has been remarked, that, from the lodgment which the Pholades have made in the pillars of the Temple of Serapis at Puteoli, it must be concluded that they have bored their holes after the erection of the pillars. Dr Bochadsch, who noticed these columns, observes, that the workmen would certainly have rejected any stones that had been disfigured in this manner. The Pholades must therefore have worked their way into them while they were buried by the influx of the sea, which immediately succeeded the destruction of the city by an earthquake. —Bochadsch, as quoted by Mr Wood.

† As Reaumur has been referred to as supporting a very different theory, I give his own words:

"Apparemment qu'il n'y a guère dans la nature de mouvement progressif plus lent que celui du Dail; muré comme il est dans son trou, il n'avance qu'en s'approchant du centre de la terre: le progrès de ce mouvement est proportionné à celui de l'accroissement de l'animal; à mesure qu'il augmente en étenduë, il creuse son trou et descend plus bas. La partie dont il se sert pour creuser ce trou est une partie charnuë située près du bout inférieur de la coquille; elle est faite en losange et assez grosse par rapport au reste du corps. Quoiqu'elle soit d'une substance molle, il n'est pas étonnant qu'elle vienne à bout de percer un trou assez profond dans une matière dure: elle y employe bien du temps. J'ai vû ces Dails se servir de cette partie à l'usage que je lui attribuë, après les avoir tirés de leurs trous et les avoir posés sur un glaise aussi molle que de la bouë; en recourbant et ouvrant ensuite cette partie, ils se creusoient un trou, et en creusoient en peu d'heures un aussi profond que celui auquel ils travaillent pendant plusieurs années, aussi y trouvoient-ils beaucoup moins de résistance, et le besoin qu'ils avoient de se cacher leur faisoit apparemment accélérer leur travail." *Mém. de l'Acad. Roy. des Sciences*, 1712, p. 127.

next professes to give an account of the manner in which the Pholades perforate their dwellings; but, from the contrariety of his statements, and his completely misunderstanding one of the authors quoted by himself, little reliance is to be placed upon his authority as an observer. In one passage of his *Zoomorphose*, when describing the shell of the *Pholas dactylus*, he says it resembles a file, with elevated striæ and asperities, dentated and crowded from the top of the shell to its base, in such a manner that the strongest points are towards the head. " It appears," says he, " that with these arms it pierces the stones, and enlarges its tomb as it increases in size." But, in a passage a little afterwards, he adds, with a strange forgetfulness of what he had previously written, " In proportion as this animal grows, it digs its hole with a round and fleshy part like a tongue; and it is not with its two valves, nor with its teeth, that it performs this operation." Further on he remarks upon another species, that it " is armed at its extremity with two strong and cutting points, in form of an auger, of which the dentated contour gives it the means of turning upon itself, and of piercing the stone downwards. The striæ and the teeth do the rest *."

Among the more modern writers, PENNANT mentions having frequently taken the Pholades " out of the cells they had formed in hard clay, below high water-mark, on many of our shores. They also perforate the hardest oak-plank that is lodged in the water. The bottoms of the cells," adds this acute observer, " are round, *and appear as if nicely turned with some instrument* †." MONTAGU, speaking of the *Mya Pholadia*, says, " It is probable this, as well as similar animals whose habits are to perforate stone, are provided with an acid, or some other solvent men-

* L'Hist. Nat. éclaircie dans une de ses parties principales.—*Zoomorphose*, p. 69, 70. Paris, 1757.

† *British Zoology*, vol. iv. p. 158.

struum capable of performing that office." And, in another passage, he observes, "The Pholades are performing similar works assigned by nature on softer substances, such as chalk, indurated clay, and wood, which, in like manner, are perforated by some solvent power :—not by the thin fragile shells that cover such animals, as some have erroneously asserted and is too generally credited *."

A late writer, Mr Wood, supports something like the same theory; at least he seems to think that the attrition of the shell is insufficient for the effect produced; "since," says he, "there are some species, and particularly the *P. orientalis*, which are nearly smooth at the anterior end, and, consequently, unfit for such a purpose †;" while Mr Gray, in the Zoological Journal, gives it as his opinion, that the Pholades "appear to bore by means of rasping ‡."

Such are the discordant opinions that have been held regarding the mode by which the Pholades perforate calcareous stones and wood: one class of naturalists asserting that they do so by the rotatory motion of their valves, or by means merely mechanical; while others suppose, from the apparent fragility of the

* *Testacea Britannica*, p. 560, 561.

"It is well known (observes Montagu in another place) that animals as well as vegetables prepare, by various occult processes, fluids powerfully corrosive: the viper secretes a deadly poison, which is forced through the cavity of its fang; the pismire, and some other insects, eject a powerful acid, capable of dissolving calcareous stone. Surely, then, it may most reasonably be admitted, that, by some such chemical means, prepared in the great elaboratory of Nature, these testaceous *Ascidia* perform the part assigned to them by the Creator of the Universe."—Montagu, *Supplement*, p. 15.

The Pholades, it is remarkable, bore the wood across the grain, while the *Teredo navalis* perforates it in the direction of the fibres.

† *General Conchology*, vol. i. p. 74.

‡ *Zoological Journal*, No. 3. p. 406.

shell, that they must have the power of secreting some solvent fluid, capable of decomposing the substances in which they burrow. That the first of these hypotheses is the one most conformable to appearances, no one who has seen the living animals can doubt, and accordingly, it has been adopted by most recent observers; while that supported by Montagu and others opposes obstacles to its reception not easily to be got over. Any acid or solvent fluid that would act with effect on the calcareous stones in which the Pholades lodge, would, it is evident, act equally on the shell of the animal itself; and a solvent which possessed the power of dissolving stone, would be little likely to have the same effect on the fibres of submerged wood.

Some years ago, while residing at Portobello, I discovered, on the coast at Joppa Salt-pans, where the rocks are uncovered at low water, numerous perforations in the shale or clay-rock, which I ascertained to be the work of Pholades. On breaking the stone in different places two species of Pholas, *P. crispata* and *candida*, were procured alive, in great numbers, and of all ages. When the tide recedes, they withdraw their tube within the perforations, but when covered by the water, its rounded mouth is visible above the upper surface of the rock. On striking the rock with a hammer, near any of the holes, a spirt of water is ejected, similar to what occurs when the Myæ and Solenes are disturbed in their haunts. The Pholades are found at various depths in the stone, corresponding to the age of the animal; the largest, and of course oldest, specimens being found at from four to six inches, or even more, under the surface; others at all intermediate distances, the youngest being merely covered by a thin layer of the clay. The *Pholas candida*, not a common species on some coasts, occurs most plentifully; but both species are frequently found together.

The perforations in the rock at the surface are not much larger in diameter than a quill; many are much smaller, but they

widen as they recede downwards, corresponding to the animal's growth. The Pholas itself is found in an inverted pear-shaped cavity at the bottom, the largest diameter of the shell being undermost. Where the Pholades are crowded together, which is generally the case, the divisions between the different cells are often extremely thin, and in some this partition is completely removed. The direction of the bore is not always vertical, though nearly so; but in some instances, where the rock had been broken down to an angle, or rounded, the Pholades were found at various inclinations, corresponding to the surfaces of the stone.

From repeated examination of the recent animals, and their perforations, I have no hesitation in asserting, that these two species, at least, form their holes by the rotatory motion or rasping of the stone with their valves. Indeed, I am surprised how any one who has seen these animals in their native rocks could for a moment think otherwise; for in the Joppa specimens, circular lines are distinctly visible in the cell of the animal, corresponding to the elevated striæ on the shell, and presenting the appearance as of having been bored by an auger. Pennant remarks the same circumstance in the cells of the Pholades found by him on the English coast, as Bonanni had formerly done in the Italian specimens. These marks, indeed, disappear in the upper part of the perforation, from the friction occasioned by the expansion and contraction of the rugous tube; but in the cavity where the Pholas lodges it is always distinctly, and often, especially when the animal is large, prominently marked.

Specimens of the shells, from the locality mentioned, are now submitted to the Society, along with portions of the shale in which they are found.

It has been held, as a presumption against the Pholades perforating rocks by a mechanical operation, that some of the species have shells nearly smooth, and unfitted for such a purpose; and the *Mya Pholadia* and *Mytilus lithophagus* are produced as

instances where it is next to impossible that, without the aid of a solvent fluid, such animals could form protecting cells in hard substances. From not having seen the animals alluded to alive, and in their native habitations, it would be presumption in me to give a decided opinion on the subject. But, reasoning from analogy in the structure of the animals, and the habits of such as have been observed, it infers no impossibility to conceive that they penetrate rocks in a similar manner. Little asperity in the instrument is required where the operation is constant. In judging of the unseen or unobserved operations of nature, many are guided in their opinion by what appears possible to be effected by the limited powers which a preconceived theory prescribes to the instrument employed. But little is known regarding the time which these instinctive miners take to form their deepening cells. A drop of water falling constantly on the same spot soon leaves evidences of what time, with the smallest force, can effect; and the keys of musical instruments are, in no long period, hollowed by the softest touch of the softest fingers. There seems no impossibility, therefore, in conceiving that the Pholades may perforate a substance less hard than their own shell by mere attrition *, or even a harder substance, by the constant action of their muscular foot.

LINNÆUS and LAMARCK regard the Pholas as a Bivalve shell, with accessory pieces; while others, from the presence of these auxiliary plates, have classed it among the Multivalves. The animal is hermaphrodite and viviparous, hatching its young in the little sacs of its branchiæ. It has a membranous mantle, of

* Of the power of the Pholades to bore limestone, marble, or shale, it is easy to satisfy one's self, by the simple experiment of rubbing the shell gently on a piece of marble, which it cuts without rounding the asperities of the shell. Oak is likewise scratched in the same manner; but the action of the Pholades is always on submerged wood or rocks partially covered by the tide, and the water, in both cases, must facilitate the process of boring.

a tubular form, open at both extremities, like that of the Solen or Mya *. From the superior opening of this tubular mantle two united syphons arise, of which the anterior is the largest. They are slightly dentated on the margin, and serve, the one for the entrance of food, and the other for discharge. When covered by the tide, or in a basin, these tubes may be seen constantly sucking in and ejecting the water. The foot is short and conical, and, from its capacity of being projected and drawn in within its circular covering, probably affixes itself by suction to the bottom of the hole, and serves as a fulcrum for the rotatory motion of the valves, or even may itself assist in deepening the cell of the animal. Mr Gray, in the third number of the Zoological Journal, has given some anatomical details regarding the structure of the Pholades, particularly with regard to the singular falciform projections in the interior of the shell, which he shews are nowise connected with the arrangement of the hinge; and Poli, in his "superb work" on the Testacea of the Two Sicilies, is said to have given the anatomy of the Pholas in detail †.

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* There are several striking points of similarity of habit between the Pholades and the Myæ. The Myæ burrow in sand, gravel, or clay, and project their tube to the surface in the same manner as the Pholades. The form of their syphon is nearly the same, as is also the mantle which connects the two valves. Their mode of sucking in the water, and expelling it in jets, is the same in both. I have kept the *Mya arenaria* alive in sea-water for several days, and witnessed repeatedly its wetting the room to a considerable distance, from its often repeated and violent ejection of the water. The Myæ, however, at least the *M. arenaria* and *truncata*, though they easily penetrate soft clay or sand, do not seem to have the power of boring into hard substances; for in many specimens I have met with, in gravelly places, the shell was distorted, from being placed between stones, which its force could neither remove nor form to the contour of its shell.

† Since the foregoing remarks were written, I have seen Poli's magnificent work, and feel gratified by finding that the observations I have hazarded entirely coincide

The Pholades being incapable of moving from their place, the young are dropped from the tube of the parent on the surface of their native rock. How they are enabled to penetrate the rock, so as to secure themselves protection; or how, previously to having formed a cell, they adhere to the surface, has not hitherto been explained. RONDELETIUS, like others of the older naturalists, who believed in spontaneous generation, supposed that the sea-water lodging in the pores of the rocks might become, in process of time, Pholades*;—a supposition not more distant from truth than that which long afterwards prevailed as to the *Lepas anatifera* being the young of a species of goose! Perhaps some glutinous matter, such as fixes the *byssus* of the Mytili, may keep the fry of the Pholades in their place till they have excavated a hole sufficient to conceal themselves: but future observation, by those who have the opportunity, will, there

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with the opinions of that able observer. In Tab. VII. he has not only given beautiful representations of the shell of the *Pholas dactylus*, and its contained animal; but has displayed its anatomical structure in a series of figures which leaves nothing further to be desired. M. POLI is clearly of opinion that the Pholades bore the rocks by mechanical action alone; and he elsewhere adduces arguments to prove that even the *Mytilus lithophagus*, the comparatively smooth shell of which seems unfitted for such a purpose, forms its dwelling in a similar manner. The passage regarding the Pholas is as follows:

"Cryptæ hujusmodi conicam formam præ se ferunt, angustiore sui parte sursum versa, per quam Molluscum Pholadem incolens tracheas pro lubitu exerit. Earum amplitudo Pholadum ætati, atque magnitudini respondere videtur: in adultis duos circiter pedes in altitudinem patet, et hiatus diameter quinque lineas minus excedit. In junioribus, mollusca tum pede exerto, ac in terebræ formam accommodato, tum etiam conchæ ministerio circa pedis apicem veluti circa axem revolutæ, cryptam profundiorem, latioremque efficiunt quemadmodum adolescunt. Tanta est motus hujusmodi efficacia, ut lapidibus simul perterebrandis par est."—J. X. POLI, *Testacea utriusque Siciliæ eorumque Historia et Anatome*, vol. i. p. 40. Parma 1791.

* "Ego crediderim in saxorum cavernulis vel vi vel natura factis, aquæ marinæ appulsu procreari atque in concham verti, quæ cavitatis sive foraminis figuram servat." —RONDELET. *De Testaceis*, lib. i. p. 49. Lugd. 1555.

is little doubt, discover the arrangement by which these animals are enabled to commence their cells.

The Pholades, it may be remarked, seem admirably constructed for the purposes of their existence, so far as these are known. Possessing but a comparatively fragile shell, which the least force would break, and, having no weapons of defence against their aquatic enemies, Nature has furnished them with the means of amply providing for this apparent deficiency, by giving them an asylum in the solid rock. Having formed their destined habitations, which they can never leave, the rock is honeycombed by successive races till it falls in pieces, and a new surface is exposed for new generations. The tribes of Pholades on the different coasts are thus active and powerful instruments in the disintegration of rocks. The shale in which they occur at Joppa runs in parallel and alternating strata, with a coarse sandstone; and while the unconnected ridges of the sandstone still appear, rounded by the weather, or hollowed into basins by the action of the waves, the alternating beds of shale have nearly disappeared, through the instrumentality of these powerful, though unseen agents *.

The Pholades are regularly used as an article of food on the coasts of France and Italy, where they abound. In the neighbourhood of Dieppe, bands of women and children, each armed

* The disposition of the strata on the coast at Joppa, and its present appearance, strikingly illustrates the power of the instruments which Nature has employed in the disintegration of certain classes of rocks. The beds of shale, which in some places seem to have been from 12 to 20 feet thick, have in most instances wholly disappeared; parallel roads or spaces, deeply covered with sand, and on a level with the neighbouring shore, being alone left to mark out the places formerly occupied by the shale. Dead shells, of very large size, are also frequently found on various parts of the coast, or dredged up by fishing-boats; thus affording indications, in the places where they are found, of the disappearance of strata effected by their agency. Encrinites are found in the shale at Joppa inhabited by the Pholades.

with a pick-axe, break the rocks inhabited by them, for the purpose of sending them to market, or as bait for fish. They are found in every sea where the rocks are suitable for their burrowing, and are met with fossil in many countries of Europe *.

* Bosc. in *Nouv. Dict. d'Hist. Nat.* Vol. xxv. p. 539. M. G. P. Deshayes has recently described and figured four species of fossil Pholades, found by him, among other perforating bivalves, at the village of Valmondois in France.—*Mém. de la Soc. d'Hist. Nat.* tom. i. p. 245.

XXXI. *Description of a new Register Thermometer, without any Index; the principle being applicable to the most delicate Mercurial Thermometers.* By H. H. Blackadder, Esq. F. R. S. E.

(Read April 17. 1826.*)*

On a former occasion, I had the honour of describing and exhibiting to this Society a new Registering Thermometer, by means of which the atmospheric temperature may be ascertained at any given instant during absence. In the construction of the instrument then described, a sliding index within the tube is indispensable; and, whenever such an index is employed, the diameter of the tube, and consequently that of its bulb, must be such as to render the instrument defective, when great accuracy and attention to minute fractions is requisite, such as in barometrical measurements, and various delicate experiments. Besides, though an instrument made with such an index may be so constructed as to perform with great accuracy, still, inasmuch as it is a complication, it is defective, and is more or less liable to error, as its construction may have been more or less perfect.

The instrument which I now mean to describe, is free from all such objections, as no index of any description is requisite, and it may be made of two of the most delicate mercurial thermometers, both tubes being attached to the same slip of ivory, but with a separate scale for each.

One of the tubes, *a*, Plate XXII. Fig. 1. is hermetically sealed as usual, and the scale also is divided and numbered in the usual manner. The other tube *d*, is not hermetically sealed, but left open at its upper extremity, which must be made flat and smooth. This in general is easily and at once effected, by making a small scratch with a sharp-edged file, previous to break-

PLATE XXII.

Fig. 1.

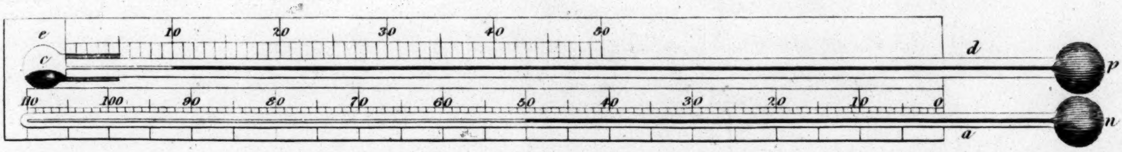
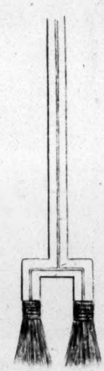


Fig. 2.

Fig. 3.

Fig. 4.



ing off a small portion of the tube. The open extremity of the tube *d*, is inserted into a portion of a larger thin glass-tube, which exactly fits it, and which terminates in a hollow bulb, containing a small quantity of mercury, *c*, Figs. 1, & 2. The inner tube is carried forward until its extremity is about opposite to that part of the outer tube where it begins to swell into a bulb; and the two tubes are then made to adhere permanently, by introducing a minute quantity of colourless varnish between them.

The scale of this tube commences from its upper open extremity, and is numbered downwards 1, 2, 3, &c. but marked as in Fig. 1, 10, 20, 30, &c.

When an instrument thus formed is held upright, the globule of mercury in the bulb *e*, Fig. 1. falls on the open extremity of the tube *d*, as represented in Fig. 2.; and if the bulb *p* be now heated by the hand, the mercury will rise in the tube, and unite with the globule *c*, with which it will remain connected as long as the instrument is kept in the upright position. If the instrument be now exposed in its upright position to the air, which has, let it be supposed, the temperature of 60°, the upper extremity of the mercury in the tube *a*, will be opposite that degree of the scale; but the mercury in the tube *d*, will still remain at the beginning of its scale, and continuous with the globule *c*. Let the instrument now be placed in a horizontal position, and the entire globule of mercury will instantly quit the open extremity of *d*, leaving the tube exactly filled with that fluid, and the globule will then take the position *c*, Fig. 1. when the instrument rests on the edge of its scale. If, from the instant the globule is thus made to quit the open extremity of the tube *d*, both of the bulbs *n*, and *p*, be kept moist with a rapidly evaporating fluid, such as ether, alcohol, &c. the mercury in both tubes will descend equally, and will remain permanently below the elevation due to the temperature of the air, as long as the

evaporating fluid is kept applied to their bulbs. The existing atmospheric temperature was supposed to be 60°; let it now be supposed that the loss of heat caused by evaporation is equal to ten degrees. The mercury in the tube *a* will then point to 50, and that in the tube *d* to 10 on their respective scales; and then $10 + 50 = 60°$, which was the temperature of the air at the instant the globule quitted the open extremity of the tube *d*, when the instrument received its horizontal position.

The way in which the instrument may be placed in a horizontal position, at any given instant during absence, was formerly described,—a pocket time-piece, and a small additional but simple piece of mechanism, being all that is requisite. A vessel for containing the evaporating fluid, fitted with a valve, and a capillary tube terminating in one or more small and soft hair brushes, Fig. 4. completes the apparatus, and which can obviously be made of such small dimensions as to be easily portable.

If the bulb at the upper extremity of the tube *d*, Fig. 1, be made of the bent form represented in Fig. 3, the instrument does not require to be moved from a horizontal position. In this case, the globule of mercury is made to quit the open extremity of the tube *d*, and fall to the bottom of the bent bulb *i*, Fig. 3, by the tube *d*, on a separate piece of ivory, being made to turn half-way round on itself, the centre of motion being a line drawn through the centre of the tube, and the extremity of the bent portion being thus made to describe the half of a circle. Or, both tubes may be attached to one slip of ivory, and the latter be made to turn half-way round its own centre, being suspended at each end, so as to admit of that motion.

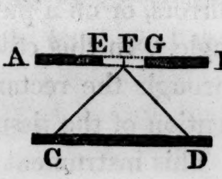
This semirotation can readily be communicated at any given instant, by the assistance of a time-piece, and a very little additional mechanism.

It is perhaps unnecessary to add, that this registering thermometer may be used as an Atmizomic Hygrometer.

XXXII. *On a new Photometer, founded on the Principles of Bouguer.* By WILLIAM RITCHIE, A. M. Rector of Tain Academy. Communicated by Dr BREWSTER.

(*Read May* 1. 1826.)

THE celebrated BOUGUER was the first who discovered the important fact, that the eye can detect a very small difference between two similar illuminated surfaces, when viewed at the same moment,—the only principle which has yet been applied with any degree of success, in determining the relative illuminating powers of artificial flames. The following is perhaps the most commodious application of this principle, to determine the relative illuminating powers of different artificial lights, particularly of coal and oil gas. The instrument, or photometer, which I employ for this purpose, is extremely simple. It consists of a rectangular box, about an inch and a half, or two inches square, open at both ends, and blackened within for the purpose of absorbing the stray-light. Within the box are placed two rectangular pieces of plane mirror, forming a right angle with each other, and cutting the sides of the box at an angle of forty-five degrees. In the upper side, or lid of the box, there is cut a rectangular opening, about an inch long, and one-eighth of an inch broad. This opening is covered with a slip of fine tissue or oiled paper. In the annexed figure, A B C D is the box; C F, F D, the two plane mirrors; E G the rectangular opening covered with a small disc of oiled or fine paper. I need hardly mention, that the two mirrors should be cut from the same plate, in order that their reflective powers may be exactly equal. The rec-



tangular slit should have a small division of blackened card at F, to prevent the possibility of the lights mingling with each other, and thus affecting the accuracy of the result.

In using this instrument, place it in the same straight line between two antagonist flames, at the distance of six or eight feet from each other; move it nearer the one or the other, till the disc of paper appear equally illuminated on each side of the middle division, and the illuminating powers of the flames will be *directly* as the squares of their distances from the middle of the photometer. In moving the instrument rapidly between the two lights, we very soon discover a boundary, on each side of which the difference between the illuminated discs becomes quite apparent. By making the instrument move from one side of this line to the other, and gradually diminishing the lengths of the oscillations, we at last place it almost exactly in its proper position. It is very convenient to have a board of the same breadth with the instrument, divided into equal parts, for the purpose of supporting it, and reading off with ease and accuracy the distances of the flames from the middle of the instrument.

In viewing the illuminated disc of paper, I use a box, about eight inches long, in the form of a prismoid, and blackened within, in order to prevent any light entering the eye, except what passes directly through the disc of paper.

Instead of the two mirrors, I sometimes use the same instrument, with a piece of white paper pasted on the faces of the mirrors, or on a piece of smooth wood, forming, as before, a right angle. In this case, the illuminated discs are viewed directly through the rectangular opening in the lid, without the intervention of the tissue or oiled paper.

This instrument is still simpler than the preceding, and in some experiments has decided advantages. But whatever form of the instrument be employed, the following precautions should

be employed, in order to insure a very close approximation to the truth. Take any number of observations, turning the instrument round at each time, and the mean of these will give a result, perhaps as accurate as the nature of the case admits; at least, it will be sufficiently accurate for all ordinary purposes.

When the colours of the flames are different, it is very difficult to ascertain the place of equal illumination. We can, however, as before, find the space over which the instrument moves, before we discover an obvious difference between the illuminated halves of the oiled or white paper. We must then take the middle of this space, which will, even in that difficult case, give us a very good approximation to the truth. The same method was also used by M. Bouguer, and found to be the best in similar cases. * But still this method is of very difficult application, when one of the lights is of a fine white, and the other of a dusky red or blue colour. In this case, I prefer the following contrivance.

Procure a piece of fine white paper, and get it printed with a small distinct type. Paste it on the rectangular opening in the instrument, which, in this case, may be somewhat enlarged. Brush over the paper with fine transparent oil, or, if the paper be very fine, this will be unnecessary. Place the instrument between the flames, and cause two assistants move them in either direction, till you can just read continuously along the paper with the same ease, and the squares of the distances will then afford a good approximation to the truth. If the second form of the instrument be used, the printed slip of paper must be pasted on the faces of the mirrors, or smooth wood, and read directly through the opening in the lid.

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* Traité d'Optique, page 50.

be employed, in order to insure a very close approximation to the truth. Take any number of observations, turning the instrument round at each time, and the mean of [illegible] will be, if not, perhaps, as accurate as the nature of the case admits, at least it will be sufficiently accurate for all ordinary purposes.

When the colours of the flames are different, it is very difficult to ascertain the place of equal illumination. We [illegible] ever, as before, find the space over which the instrument [illegible] be [illegible] sufficient difference between the illuminated halves of the [illegible]. We may [illegible] middle of this space [illegible] in that [illegible] as a very good approximation to the truth [illegible] was also used by [illegible] and found to be [illegible] in cases. [illegible] But still this method is of very [illegible] when one of the lights [illegible] deeply red [illegible] candles.

[illegible] a [illegible] small distance [illegible] instrument, which, in this case, may [illegible] Brush over the paper with [illegible] be very fine, that [illegible] between the [illegible] direction, [illegible] with the same [illegible] afford a good [illegible] of the instrument [illegible] pasted on the face of [illegible] smooth wood, and [illegible] directly through the opening in [illegible] lid.

[illegible]

HISTORY OF THE SOCIETY.

LAWS

OF THE

ROYAL SOCIETY OF EDINBURGH,

ENACTED 23D MAY 1811, AND ALTERED ON THE 26TH FEBRUARY 1820, 24TH JANUARY 1823, 13TH JANUARY 1824, AND 9TH JANUARY 1826.

I.

THE ROYAL SOCIETY OF EDINBURGH shall consist of Ordinary, Foreign, and Honorary Members.

II.

Every Ordinary Member, within three months after his election, shall pay, as fees of admission, Five Guineas, and shall further be bound to pay annually the snm of Three Guineas, into the hands of the Treasurer.

All Members who shall have paid Twenty-five years' annual subscriptions, shall be exempted from future payments.

III.

Members shall be at liberty to compound for their annual subscription, each paying according to the value of an annuity on his life, determined as in the ordinary insurance on lives.

The power of raising the admission-fee and the annual subscription shall remain with the Society.

IV.

Ordinary Members, not residing in Edinburgh, and not compounding for their annual subscription, shall appoint some person residing in Edinburgh, by whom the payment of the said subscription is to be made, and shall signify the same to the Treasurer.

V.

Members failing to pay their subscription for three successive years, due application having been made to them by the Treasurer, shall cease to be Members of the Society, and the legal means for recovering such arrears shall be employed.

VI.

None but Ordinary Members are to bear any office in the Society, or to vote in the choice of Members or Office-bearers, or to interfere in the patrimonial interests of the Society.

VII.

The number of Ordinary Members shall be unlimited.

VIII.

The *Ordinary* Members, upon producing an order from the Treasurer, shall hereafter be entitled to receive from the publisher, *gratis*, the Parts of the Society's Transactions which shall be published subsequent to their admission.

IX.

The Society having formerly admitted as Non-resident Members, gentlemen residing at such a distance from Edinburgh as to be unable regularly to attend the Meetings of the Society, with power to such Non-resident Members, when occasionally in Edinburgh, to be present at the Society's Meetings, and to take a part in all their inquiries and proceedings, without being subjected to any contribution for defraying the expences of the Society; it is hereby provided, that the privileges of such

Non-resident Members already elected shall remain as before; but no Ordinary Members shall be chosen in future under the title and with the privileges of Non-resident Members. The Members at present called Non-resident shall have an option of becoming Ordinary Members; if they decline this, they shall continue Non-resident as formerly.

X.

The *Foreign* Members shall not be subject to the Annual Contributions, nor to any Fee on admission. They shall be limited to the number of Thirty-six, and shall consist of Foreigners distinguished in Science and Literature.

XI.

The *Honorary* Members shall not be subject to the Annual Contribution, nor to any Fee on admission. They shall be limited to the number of Twenty-one, and shall consist of Gentlemen eminently distinguished in Science and Literature.

XII.

The Election of Members shall take place on the 1st Mondays of every month during the Session, at the ordinary meetings of the Society, which shall be considered as General Meetings for the Election of Members. The election shall be by ballot, and shall be determined by a majority of *Two-thirds* of the Members, provided Twenty-four Members are present, and vote.

XIII.

No person shall be proposed as an Ordinary Member, without a recommendation subscribed by *One* Ordinary Member, to the purport below *.

* "A. B. a gentleman well skilled in several branches of Science (or Polite Literature, as the case may be), being to my knowledge desirous of becoming a Member of the Royal Society of Edinburgh, I hereby recommend him as deserving of that honour, and as likely to prove an useful and valuable Member."

This recommendation shall be delivered to the Secretary, and by him laid before the Council, and shall afterwards be read at each of three ordinary meetings of the Society, previous to the day of the election, and shall lie upon the table during that time.

XIV.

Any *Three* Members may transmit, through the Secretary to the Council, recommendations of Foreign and Honorary Members. Foreign and Honorary Members may also be proposed by the Council, and they shall be elected in the same manner as the Ordinary Members.

XV.

The Classes shall meet alternately on the first and third Mondays of every month, from November to June inclusive. It shall be competent, however, to bring matters of a Physical or Literary kind, before either Class of the Society indiscriminately. To facilitate this, one Minute-book shall be kept for both Classes; the Secretaries of the respective Classes either doing the duty alternately, or according to such agreement as they may find it convenient to make.

XVI.

The Society shall from time to time make a publication of its Transactions and Proceedings. For this purpose the Council shall select and arrange the papers which they shall deem worthy of publication in the *Transactions* of the Society, and shall superintend the printing of the same.

The Transactions shall be published in Parts or *Fasciculi,* and the expence shall be defrayed by the Society.

XVII.

There shall be elected annually, for conducting the publications and regulating the private business of the Society, a Council, consisting of a President; Four Vice-Presidents, two of whom shall be resident; a President for each Class of the Society; Six Counsellors for each Class; one

Secretary for each; a Treasurer; a General Secretary; and a Curator of the Museum and Library.

XVIII.

The election of the Office-bearers shall be on the fourth Monday of November.

XIX.

Four Counsellors, Two from each class, shall go out annually. They are to be taken according to the order in which they at present stand on the list of the Council.

XX.

The Treasurer shall receive and disburse the money belonging to the Society, granting the necessary receipts, and collecting the money when due.

He shall keep regular accounts of all the cash received and expended, which shall be made up and balanced annually; and at a General Meeting, to be held on the last Monday of January, he shall present the accounts for the preceding year, duly audited. At this Meeting the Treasurer shall also lay before the Society a list of all arrears due above twelve months, and the Society shall thereupon give such directions as they may find necessary for recovery thereof.

XXI.

At the General Meeting in November, a Committee of Three Members shall be chosen to audit the Treasurer's accounts, and give the necessary discharge of his intromissions.

The report of the examination and discharge shall be laid before the Society at the General Meeting in January, and inserted in the records.

XXII.

The General Secretary shall take down minutes of the proceedings of the General Meetings of the Society and of the Council, and shall enter them in two separate books. He shall keep a list of the Donations made to the Society, and take care that an account of such Donations be published in the Transactions of the Society. He shall, as directed by the Council, and with the assistance of the other Secretaries, superintend the publications of the Society.

XXIII.

A Register shall be kept by the Secretary, in which copies shall be inserted of all the Papers read in the Society, or abstracts of those Papers, as the Authors shall prefer; no abstract or paper, however, to be published without the consent of the Author. It shall be understood, nevertheless, that a person choosing to read a paper, but not wishing to put it into the hands of the Secretary, shall be at liberty to withdraw it, if he has beforehand signified his intention of doing so.

For the above purpose, the Secretary shall be empowered to employ a Clerk, to be paid by the Society.

XXIV.

Another register shall be kept, in which the names of the Members shall be enrolled at their admission, with the date.

XXV.

A Seal shall be prepared and used, as the Seal of the Society.

XXVI.

The Curator of the Museum and Library shall have the custody and charge of all the Books, Manuscripts, objects of Natural History, Scientific

Productions, and other articles of a similar description belonging to the Society; he shall take an account of these when received, and keep a regular catalogue of the whole, which shall lie in the Hall, for the inspection of the Members.

XXVII.

All articles of the above description shall be open to the inspection of the Members, at the Hall of the Society, at such times, and under such regulations, as the Council from time to time shall appoint.

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LIST

OF THE

OFFICE-BEARERS AND MEMBERS ELECTED SINCE MARCH 3. 1823.

May 5. 1823.

MEMBERS ELECTED.

ORDINARY.

Capt. THOMAS DAVID STEWART, Hon. E. I. Comp. Service.

ANDREW FYFE, M. D.

ROBERT BELL, Esq. Advocate.

June 2. 1823.

MEMBERS ELECTED.

ORDINARY.

Capt. NORWICH DUFF, R. N.

WARREN HASTINGS ANDERSON, Esq.

LISCOMBE JOHN CURTIS, Esq. Ingsdon House, Devonshire.

ALEXANDER THOMSON, Esq. of Banchory, Advocate.

November 24. 1823.

OFFICE-BEARERS.

Sir WALTER SCOTT, Bart. President.

VICE-PRESIDENTS.

Right Hon. LORD CHIEF BARON. Dr T. C. HOPE.
Lord GLENLEE. Professor RUSSELL.

Dr BREWSTER, General Secretary.

THOMAS ALLAN, Esq. Treasurer.

JAMES SKENE, Esq. Curator of the Museum.

PHYSICAL CLASS.

ALEXANDER IRVING, Esq. President.

JOHN ROBISON, Esq. Secretary.

Counsellors from the Physical Class.

Sir JAMES HALL, Bart. ROBERT STEVENSON, Esq.
Dr KENNEDY. Sir. W. ARBUTHNOT, Bart.
Rev. Dr MACKNIGHT. JAMES JARDINE, Esq.

LITERARY CLASS.

HENRY MACKENZIE, Esq President..

P. F. TYTLER, Esq. Secretary.

Counsellors from the Literary Class.

THOMAS THOMSON, Esq. Professor WILSON.
GEORGE FORBES, Esq. Sir W. HAMILTON, Bart.
Lord MEADOWBANK. Rev. Dr LEE.

December 1. 1823.

MEMBERS ELECTED.

FOREIGN.

M. THENARD, Member of the Institute, and Professor of Chemistry in the College of France.

ORDINARY.

ROBERT KNOX, M. D.

ROBERT CHRISTISON, M. D. Professor of Medical Jurisprudence.

GEORGE KELLIE, M. D. Leith.

January 19. 1824.

MEMBERS ELECTED.

HONORARY.

The Rev. JOHN BRINKLEY, D. D., F. R. S., and President of the Royal Irish Academy.

W. H. WOLLASTON, M. D., F. R. S. &c. &c.

FOREIGN.

WILLIAM HAIDINGER, Esq. Vienna.

ORDINARY.

GEORGE HARVEY, Esq. Plymouth.

Dr LAWSON WHALLEY, Lancaster.

WILLIAM BELL, Esq. W. S. Edinburgh.

JAMES HAMILTON *jun.* M. D. Professor of Midwifery in the University of Edinburgh.

ROBERT GROAT, M. D. Edinburgh.

ROBERT GRANT, M. D. Edinburgh.

CLAUD RUSSELL, Esq. W. S. Edinburgh.

H. W. WILLIAMS, Esq. Edinburgh.

Rev. WILLIAM MUIR, D. D. one of the Ministers of Edinburgh.

February 2. 1824.

MEMBERS ELECTED.

ORDINARY.

ALEXANDER MUNRO, Esq. Edinburgh.

W. H. PLAYFAIR, Esq. Architect, Edinburgh.

March 1. 1824.

MEMBERS ELECTED.

ORDINARY.

JOHN ARGYLE ROBERTSON, Esq. Surgeon, Edinburgh.

JAMES PILLANS, Esq. Leith.

Dr MACWHIRTER, Edinburgh.

JAMES WALKER, Esq. Civil Engineer.

WILLIAM NEWBIGGING, Esq. Surgeon, Edinburgh.

May 3. 1824.

MEMBERS ELECTED.

ORDINARY.

WILLIAM WOOD, Esq. President of the Royal College of Surgeons.

WILLIAM CROSBIE MAIR, M. D. Physician to the Embassy to Mexico.

November 22. 1824.

OFFICE-BEARERS.

Sir WALTER SCOTT, Bart. President.

VICE-PRESIDENTS.

Right Hon. Lord CHIEF-BARON. Dr T. C. HOPE.

Lord GLENLEE. Professor RUSSELL.

Dr BREWSTER, General Secretary.

THOMAS ALLAN, Esq. Treasurer.

JAMES SKENE, Esq. Curator of the Museum.

PHYSICAL CLASS.

ALEXANDER IRVING, Esq. President.

JOHN ROBISON, Esq. Secretary.

Counsellors from the Physical Class.

Rev. Dr MACKNIGHT. JAMES JARDINE, Esq.

ROBERT STEVENSON, Esq. Sir WILLIAM FORBES, Bart.

Sir WILLIAM ARBUTHNOT, Bart. Dr HOME.

LITERARY CLASS.

HENRY MACKENZIE, Esq. President.
P. F. TYTLER, Esq. Secretary.

Lord MEADOWBANK.
Professor WILSON.
Sir W. HAMILTON, Bart.
Rev. Dr LEE.
Right Hon. the Lord ADVOCATE.
Dr HOME.

December 6. 1824.

MEMBERS ELECTED.

ORDINARY.

JOHN CAMPBELL, M. D. Edinburgh.
GEORGE ANDERSON, Esq. Inverness.

January 3. 1825.

MEMBERS ELECTED.

HONORARY.

ROBERT BROWN, Esq. F. R. S. London.

February 7. 1825.

MEMBERS ELECTED.

ORDINARY.

Major LEITH HAY of Rannes.
Rev. JOHN WILLIAMS, Rector of the Edinburgh Academy.
JOHN HUGH MACLEAN, Esq. Advocate.

March 7. 1825.

MEMBERS ELECTED.

FOREIGN.

M. MITSCHERLICH, Professor of Chemistry in the University of Berlin.

M. GUSTAVUS ROSE, Professor of Mineralogy in the University of Berlin.

ORDINARY.

WILLIAM PRESTON LAUDER, M. D. Edinburgh.

Right Honourable Lord RUTHVEN.

EDWARD TURNER, M. D. Lecturer on Chemistry, and Fellow of the Royal College of Physicians, Edinburgh.

April 4. 1825.

MEMBERS ELECTED.

ORDINARY.

Right Honourable Lord BELHAVEN.

Dr REID CLANNY, Physician, Sunderland.

November 28. 1825.

OFFICE-BEARERS.

Sir WALTER SCOTT, Bart. President.

VICE-PRESIDENTS.

Right Hon. Lord CHIEF-BARON. Dr T. C. HOPE.

Lord GLENLEE. Professor RUSSELL.

Dr Brewster, General Secretary.

Thomas Allan, Esq. Treasurer.

James Skene, Esq. Curator of the Museum.

PHYSICAL CLASS.

Alexander Irving, Esq. President.

John Robison, Esq. Secretary.

Counsellors of the Physical Class.

Sir William Arbuthnot, Bart.	Dr Home.
James Jardine, Esq.	Professor Wallace.
Sir William Forbes, Bart.	Dr Edward Turner.

LITERARY CLASS.

Henry Mackenzie, Esq. President.

P. F. Tytler, Esq. Secretary.

Sir W. Hamilton, Bart.	Sir Henry Jardine.
Rev. Dr Lee.	Sir John Hay, Bart.
Right Hon. the Lord Advocate.	Dr Hibbert.

December 5. 1825.

MEMBERS ELECTED.

Ordinary.

John A. Stewart, Esq. younger of Grandtully.

James Hall, Esq. Advocate.

January 9. 1826.

MEMBERS ELECTED.

ORDINARY.

HENRY HOME BLACKADDER, Esq. Surgeon, Edinburgh.

At this Meeting it was agreed, " That Law No. XII. be altered, so that hereafter no Candidate, when ballotted for, shall be considered as admitted, unless there be a majority of at least Two-thirds of the Votes in his favour."

February 6. 1826.

MEMBERS ELECTED.

ORDINARY.

ALEXANDER WOOD, Esq. Advocate.

Rev. DIONYSIUS LARDNER, Fellow of Trinity College, Dublin.

March 6. 1826.

MEMBERS ELECTED.

ORDINARY.

GEORGE MACPHERSON GRANT, Esq. M. P. of Ballindalloch.

WILLIAM RENNY, Esq. W. S. Solicitor of Stamps.

ELIAS CATHCART, Esq. Advocate.

April 3. 1826.

MEMBERS ELECTED.

ORDINARY.

ANDREW CLEPHANE, Esq. Advocate.

May 1. 1826.

MEMBERS ELECTED.

ORDINARY.

Rev. GEORGE COVENTRY.

Sir DAVID HUNTER BLAIR, Bart.

FOREIGN.

G. MOLL, Professor of Natural Philosophy in the University of Utrecht.

M. STROMEYER, Professor of Chemistry in the University of Gottingen.

M. HAUSMANN, Professor of Mineralogy in the University of Gottingen.

LIST *of the Present* ORDINARY MEMBERS *of the* ROYAL SOCIETY OF EDINBURGH, *in the order of their Election.*

His Majesty THE KING Patron.

Date of Election.

Andrew Duncan *senior*, M. D. *Professor of the Theory of Physic.*
Dr James Hamilton *senior*, *Physician, Edinburgh.*
Sir William Miller, Baronet, Lord Glenlee.
James Russell, Esq. *Professor of Clinical Surgery.*
Charles Stuart, M. D. of Dunearn, *Physician, Edinburgh.*
Dugald Stewart, Esq.

The above Gentlemen were Members of the Edinburgh Philosophical Society.

1783. Honorable Lord Hermand.
Honorable Baron Hume.
Henry Mackenzie, Esq.
Honorable Lord Bannatyne.
Reverend William Trail, LL. D. *Chancellor of St Saviour's, Connor.*

The above Gentlemen were associated with the Members of the Philosophical Society at the Institution of the Royal Society in 1783.

The following Members were regularly elected.

1784. Sir James Hall, Baronet, F. R. S. Lond.
Honorable Lord Eldin.
Reverend Archibald Alison, LL. B. *Edinburgh.*
1785. James Hare, M. D. *late of Calcutta.*
1786. Robert Blair, M. D. *Professor of Practical Astronomy.*
1787. James Home, M. D. *Professor of the Practice of Physic.*

Date of Election.

1788. Thomas Charles Hope, M. D. F. R. S. Lond. *Professor of Chemistry.*
Right Honorable Charles Hope, *Lord President of the Court of Session.*
1792. Andrew Coventry, M. D. *Professor of Agriculture.*
1793. Sir Alexander Muir Mackenzie, Bart *of Delvin.*
1795. The very Reverend Dr George Husband Baird, *Principal of the University.*
Robert Hamilton, Esq. *Professor of Public Law.*
1796. General Dirom, *of Mount Annan,* F. R. S. Lond.
Reverend Sir Henry Moncrieff Wellwood, Baronet.
The Honorable Baron Sir Patrick Murray, Baronet.
Andrew Berry, M. D. *Edinburgh.*
1797. Andrew Duncan *junior,* M. D. *Professor of Materia Medica.*
1798. Alexander Monro, M. D. *Professor of Anatomy, &c.*
Right Honorable Sir John Sinclair, Bart.
1799. Reverend Thomas Macknight, D. D.
Honorable Lord Robertson.
Sir George S. Mackenzie, Baronet, F. R. S. Lond.
Robert Jameson, Esq. *Professor of Natural History.*
1800. Sir William Arbuthnot, Bart.
Gilbert Innes, Esq. *of Stow.*
Sir Walter Scott, Baronet, *of Abbotsford.*
Colonel D. Robertson Macdonald.
1803. Reverend John Jamieson, D. D.
Thomas Telford, Esq. *Civil Engineer.*
James Bryce, Esq. *Surgeon, Edinburgh.*
Reverend Dr Andrew Brown, *Professor of Rhetoric.*
1804. William Wallace, Esq. *Professor of Mathematics.*
Sir William Forbes, Bart. *of Pitsligo.*
Alexander Irving, Esq. *Professor of Civil Law.*
1805. Thomas Allan, Esq. F. R. S. Lond.
Thomas Thomson, M. D. F. R. S. Lond. *Professor of Chemistry, Glasgow.*
1806. Robert Ferguson, Esq. *of Raith,* F. R. S. Lond.
George Bell, Esq. *Surgeon, Edinburgh.*
George Dunbar, Esq. *Professor of Greek.*
1807. Sir James Montgomery, Baronet, *of Stanhope,* M. P.
John Barclay, M. D. *Lecturer on Anatomy, &c.*
John Leslie, Esq. *Professor of Natural Philosophy.*
John Campbell, Esq. *of Carbrook.*

Date of Election.

Thomas Thomson, Esq. *Advocate.*
William Fraser Tytler, Esq. *Advocate.*
1808. James Wardrop, Esq. *Surgeon Extraordinary to his Majesty.*
David Brewster, LL. D. F. R. S. Lond.
1810. Reverend Dr William Ritchie, *Professor of Divinity.*
1811. Charles Bell, Esq. *Surgeon, London.*
Alexander Nimmo, Esq. *Civil Engineer.*
Reverend Andrew Stewart, M. D. *Erskine.*
Reverend David Ritchie, D. D. *Professor of Logic.*
Major-General Sir Thomas Brisbane Makdougal, K. C. B.
1812. Right Honorable Lord Gray, F. R. S. Lond.
General Dyce.
John Thomson, M. D. *Physician, Edinburgh.*
James Jardine, Esq. *Civil Engineer.*
Captain Basil Hall, R. N. F. R. S. Lond.
J. G. Children, Esq. F. R. S. Lond.
Alexander Gillespie, Esq. *Surgeon, Edinburgh.*
W. A. Caddell, Esq. F. R. S. Lond.
Macvey Napier, Esq. F. R. S. Lond.
James Pillans, Esq. *Professor of Humanity.*
Sir George Clerk, Bart. M. P. and F. R. S. Lond.
Daniel Ellis, Esq. *Edinburgh.*
1813. William Somerville, M. D. F. R. S. *London.*
James Hare, *jun.* M. D. *late of Calcutta.*
Henry Davidson, M. D. *Physician in Edinburgh.*
1814. Sir Henry Jardine, *King's Remembrancer in Exchequer.*
Patrick Neill, Esq. *Secretary to the Wernerian and Horticultural Societies.*
Right Honorable Lord Viscount Arbuthnot.
Reverend John Thomson, *Duddingston.*
Reverend John Fleming, D. D. *Flisk.*
John Cheyne, M. D. *Physician, Dublin.*
Sir James Mackintosh, M. P. *London.*
Lieut.-Colonel Tytler, *Edinburgh.*
Reverend Alexander Brunton, D. D. *Professor of Oriental Languages.*
Professor George Glennie, *Marischall College, Aberdeen.*
1815. Gilbert Laing Meason, Esq. *of Lindertis.*
Robert Stevenson, Esq. *Civil Engineer.*
Sir Thomas Dick Lauder, Bart. *of Fountainhall.*
John Yule, M. D. *Physician in Edinburgh.*

Date of
Election.

Henry Home Drummond, Esq. *of Blair-Drummond*, M. P.
Charles Granville Stewart Menteath, Esq. *of Closeburn.*
William Thomas Brande, Esq. Sec. R. S. Lond. and *Professor of Chemistry in the Royal Institution.*
1816. Colonel Thomas Colby, F. R. S. *Royal Engineers.*
Leonard Horner, Esq. F. R. S. Lond.
Henry Colebrooke, Esq. *Director of the Asiatic Society of Great Britain.*
Reverend George Cook, D. D. *Laurencekirk.*
Right Honorable William Adam, *Lord Chief Commissioner.*
John Fullerton, Esq. *Advocate.*
Thomas Jackson, LL. D. *Professor of Natural Philosophy, St. Andrew's.*
John Robison, Esq. *Edinburgh.*
Hugh Murray, Esq. *Edinburgh.*
1817. The Honorable Baron Clerk Rattray.
Right Honorable the Earl of Wemys and March.
Francis Hamilton, M. D. F. R. S. and F. A. S. Lond.
John Wilson, Esq. *Professor of Moral Philosophy.*
Honorable Lord Meadowbank.
John Fleming, M. D. *late of Calcutta.*
James Hamilton Dickson, M. D. *Clifton.*
William P. Alison, M. D. *Professor of the Theory of Physic.*
James Skene, Esq. *of Rubislaw.*
John Howell, M. D.
Reverend Robert Morehead, *Edinburgh.*
Robert Bald, Esq. *Civil Engineer.*
Thomas Sivright, Esq. *of Meggetland.*
1818. William Richardson, M. D. *Physician, Harrowgate.*
Right Honorable Lord Napier.
Harry William Carter, M. D. *Oxford.*
Patrick Miller, M. D. *Exeter.*
John Craig, Esq. *Edinburgh.*
John Watson, M. D.
Captain Thomas Brown, F. L. S.
John Hope, Esq. *His Majesty's Solicitor-General.*
Major James Alston *of Auchenard.*
William Ferguson, M. D. *Windsor.*
Sir William Hamilton, Bart. *Professor of Civil History.*
1819. Right Honorable Lord John Campbell, F. R. S. Lond. and M. R. I.
Sir John Hay, Bart. *of Smithfield and Hayston.*

Date of Election.

Dr Shoolbred, *Calcutta.*
Patrick Fraser Tytler, Esq. *Advocate.*
Major-General David Stewart *of Garth.*
Patrick Murray, Esq. *of Simprim.*
James Muttlebury, M. D. *Bath.*
Thomas Stewart Traill, M. D. *Liverpool.*
Alexander Kennedy, M. D. *Physician, Edinburgh.*
Mr Alexander Adie, *Optician, Edinburgh.*
William Couper, M. D. *Glasgow.*
John Hennen, M. D.
John Veitch, M. D.
Andrew Waddel, Esq. *Hermitage Hill.*
Marshall Hall, M. D. *Nottingham.*
John Borthwick, Esq. *Advocate.*
Richard Phillips, Esq. F. R. S. *London.*
Rev. William Scoresby.
George Forbes, Esq. *Edinburgh.*
1820. James Hunter, Esq. *of Thurston.*
Right Honorable David Boyle, *Lord Justice-Clerk.*
James Keith, Esq. *Surgeon, Edinburgh.*
Right Honorable Sir Samuel Shepherd, *Lord Chief-Baron.*
James Nairne, Esq. W. S. *Edinburgh.*
John Colquhoun, Esq. *Advocate.*
Lieutenant-Colonel M. Stewart.
Charles Babbage, Esq. F. R. S. Lond.
Thomas Guthrie Wright, Esq. *Auditor of the Court of Session.*
John F. W. Herschel, Esq. Sec. R. S. Lond.
Adam Anderson, Esq. A. M. *Rector of the Academy, Perth.*
John Shank More, Esq. *Advocate.*
Dr George Augustus Borthwick, *Surgeon, Edinburgh.*
Robert Dundas, Esq. *of Arniston.*
Samuel Hibbert, M. D. *and Sec. to the Society of Scottish Antiquaries, Edin.*
Rev. Robert Haldane, D. D. *Principal of St Mary's College, St Andrew's.*
Sir John Meade, M. D. *Weymouth.*
Thomas Kinnear, Esq. *Edinburgh.*
Dr William Macdonald *of Ballyshear.*
John Hall, Esq. *younger of Dunglass.*
Admiral Adam.

Date of Election.

John Hay, Esq. *younger of Smithfield and Hayston.*
George Ballingall, M. D. *Professor of Military Surgery.*
1821. Major-General Straton, C. B. &c. &c.
Robert Graham, M. D. *Professor of Botany.*
A. N. Macleod, Esq. *of Harris.*
Sir James M. Riddell, Bart. *of Ardnamurchan.*
Archibald Bell, Esq. *Advocate.*
John Clerk Maxwell, Esq. *Advocate.*
John H. Wishart, Esq. *Surgeon, Edinburgh.*
John Lizars, Esq. *Surgeon, Edinburgh.*
Edward Earl, Esq.
John Cay, Esq. *Advocate.*
Sir Charles Gieséckè, *Professor of Mineralogy to the Dublin Society.*
Robert Kaye Greville, LL. D. *Edinburgh.*
Robert Hamilton, M. D. *Edinburgh.*
Robert Allan, Esq. *Surgeon, Edinburgh.*
Sir Archibald Campbell, Bart.
Sir David Milne, K. C. B.
Colonel Mair, *Deputy-Governor of Fort-George.*
A. R. Carson, Esq. *Rector of the High School.*
James Buchan, M. D. *Physician, Edinburgh.*
James Tytler, Esq. *of Woodhouselee, W. S.*
1822. Francis Chantry, Esq. F. R. S. *London*, &c.
Edward Troughton, Esq. F. R. S. *London*, &c.
James Smith, Esq. *of Jordanhill.*
William Bonar, Esq. *Edinburgh.*
Colin Mackenzie, Esq. *Deputy Keeper of the Signet.*
Rev. H. Parr Hamilton, *Cambridge.*
Captain J. D. Boswall, R. N. of *Wardie.*
Dr John Aitken, *Physician, Edinburgh.*
James Graham, Esq. *Advocate.*
George A. Walker Arnott, Esq. *Advocate.*
Rev. John Lee, M. D. *Edinburgh.*
John Ayton, Esq. *of Inchdarnie.*
Richard Saumarez, Esq.
James South, Esq. F. R. S. *London.*
Lieutenant-Colonel Martin Whyte, *Edinburgh.*

Date of Election.

Walter Frederick Campbell, Esq. *of Shawfield*, M. P.
George Joseph Bell, Esq. *Professor of Scots Law.*
Dr William Dyce, *Aberdeen.*
W. C. Trevelyan, Esq. *Wallington.*
Robert Abercromby, Esq. *younger of Birkenbog.*
Dr Shortt, *Edinburgh.*
Dr Wallich, *Calcutta.*
1823. The Right Honorable Sir George Warrender, Bart. *of Lochend.*
John Russell, Esq. W. S. *Edinburgh.*
John Shaw Stewart, Esq. *Advocate.*
Alexander Hamilton, M. D. *Physician, Edinburgh.*
Thomas Harland, M. D. *Physician, Scarborough.*
John Dewar, Esq. *Advocate, Edinburgh.*
Right Honorable Sir William Rae, Bart. *of St Catherine's, Lord Advocate.*
Sir Robert Dundas, Bart. *of Beechwood.*
William Cadell, Esq. *of Cockenzie.*
Sir William Knighton, Bart.
Sir Edward French Bromhead, Bart. A. M. F. R. S. Lond., *Thurlsby Hall.*
Sir James Stuart, Bart. *of Allanbank.*
Sir Andrew Halliday, *Physician to His Royal Highness the Duke of Clarence.*
John Bonar, Esq. *younger of Kimmerghame.*
Alexander Waddell, Esq. *Hermitage Hill.*
Captain Thomas David Stuart *of the Hon. East India Company's Service.*
Andrew Fyfe, M. D. *Lecturer on Chemistry, Edinburgh.*
Robert Bell, Esq. *Advocate.*
Captain Norwich Duff, R. N.
Warren Hastings Anderson, Esq.
Alexander Thomson, Esq. of Banchory, *Advocate.*
Liscombe John Curtis, Esq. *Ingsdon House, Devonshire.*
Robert Knox, M. D. *Lecturer on Anatomy, Edinburgh.*
Robert Christison, M. D. *Professor of Medical Jurisprudence.*
John Gordon, Esq. *of Cairnbulg.*
George Kelly, M. D. *Leith.*
1824. George Harvey, Esq. F. R. S. Lond. *Plymouth.*
Dr Lawson Whalley, *Lancaster.*
William Bell, Esq. W. S. *Edinburgh.*
Dr Wilson Philip, *Bristol.*

Date of Election.

Dr James Hamilton *jun., Professor of Midwifery in the University of Edinburgh.*
Dr Robert Grant, *Edinburgh.*
Claud Russell, Esq. *Accountant, Edinburgh.*
H. W. Williams, Esq. *Edinburgh.*
Rev. Dr William Muir, *one of the Ministers of Edinburgh.*
Alexander Munro, Esq. *Edinburgh.*
W. H. Playfair, Esq. *Architect, Edinburgh.*
John Argyle Robertson, Esq. *Surgeon, Edinburgh.*
James Pillans, Esq. *Leith.*
Dr Macwhirter.
James Walker, Esq. *civil engineer.*
William Newbigging, Esq. *surgeon.*
William Wood, Esq. *Surgeon, Edinburgh.*
Dr William Crosbie Mair, *Physician to the Embassy to Mexico.*
John Campbell, M. D. *Edinburgh.*
George Anderson, Esq. *Inverness.*
1825. Major Leith Hay, *of Rannes.*
Rev. John Williams, *Rector of the Edinburgh Academy.*
John Hugh Maclean, Esq. *Advocate.*
Dr W. Preston Lauder.
Right Honorable Lord Ruthven.
Edward Turner, M. D. *Lecturer on Chemistry, and Fellow of the Royal College of Physicians.*
Right Honorable Lord Belhaven.
Dr Reid Clanny, *Sunderland.*
John Archibald Stewart, Esq. younger of Grandtully.
James Hall, Esq. *Advocate.*
Sir William Jardine, Bart. of Applegarth.
H. H. Blackadder, Esq. *Surgeon, Edinburgh.*
Alexander Wood, Esq. *Advocate.*
Rev. Dionysius Lardner, *Trinity College, Dublin.*
1826. George Macpherson Grant, Esq. M. P. *of Ballindalloch.*
William Renny, Esq. W. S. *Solicitor of Stamps.*
Elias Cathcart, Esq. *Advocate.*
Andrew Clephane, Esq. *Advocate.*
Rev. George Coventry.
Sir David Hunter Blair, Bart.

LIST *of* NON-RESIDENT *and* FOREIGN MEMBERS *elected under the Old Laws.*

Sir Gilbert Blane, M. D. F. R. S. *London.*
John Hunter, LL. D. *Professor of Humanity, St Andrew's.*
George Jardine, A. M. *Professor of Logic, Glasgow.*
Right Honorable the Earl of Morton.
Right Honorable the Earl of Dundonald.
Right Honorable Sir Robert Liston, Bart.
Mr Jefferson.
M. Le Chevalier, *Paris.*
Dr S. L. Mitchell, *New York.*
Right Honorable Thomas Wallace, Esq. *of Carlton Hall.*
Rev. Thomas Somerville, D. D. *Jedburgh.*
John Gillies, LL.D. *Historiographer to his Majesty.*
Robert Freer, M. D. *Professor of the Theory and Practice of Physic, Glasgow.*
M. P. Prevost, *Geneva.*
Rev. Walter Fisher, *Cranston.*
Rev. Bishop Gleig, *Stirling.*
Charles Hatchet, Esq. F. R. S. Lond.
Major Rennel, F. R. S. Lond.
Sir Henry Stuart, Bart. *of Allanton.*
Sir William Blizzard, M. D. F. R. S. Lond.
Thomas Blizzard, Esq.
Sir William Ouseley, Bart.
The Right Honorable the Earl of Traquair.
Sir William Drummond, Bart. *of Logie Almond.*
Sir James Macgrigor, M. D.
Richard Chenevix, Esq. F. R. S. Lond.
Richard Griffiths, Esq. *Civil Engineer.*

LIST *of* HONORARY *and* FOREIGN MEMBERS, *elected under the New Laws.*

HONORARY.

The Marquis de Laplace, *Member of the Institute of France.*
Baron Cuvier, *Secretary to the Institute of France.*
M. Humboldt, *Member of the Institute of France.*
Sir Humphry Davy, Bart. P. R. S. Lond.
M. Gay Lussac, *Member of the Institute of France.*
M. Biot, *Member of the Institute of France.*
M. Arago, *Member of the Institute of France.*
His Royal Highness Prince Leopold.
His Royal Highness the Archduke Maximilian.

The above Members were elected before the new class of Foreign Members was established.

His Imperial Highness the Archduke John of Austria.
M. Le Chevalier Joseph Hammer.
M. Goethé.
Rev. Dr Brinkley, F. R. S. Lond., *and President of the Royal Irish Academy.*
Dr W. H. Wollaston, F. R. S. Lond. &c. &c.
Robert Brown, Esq. F. R. S. Lond. &c. &c.

FOREIGN.

M. Le Chevalier Legendre, *Member of the Institute of France.*
M. Poisson, *Member of the Institute of France.*
M. Vauquelin, *Member of the Institute of France.*
M. Prony, *Member of the Institute of France.*
M. Brochant, *Member of the Institute of France.*
Baron Leopold Von Buch, *Berlin.*
M. Gauss, *Professor of Mathematics, Göttingen.*
M. Blumenbach, *Professor of Natural History, Göttingen.*

Jacob Berzelius, M. D. F. R. S. Lond. *Professor of Chemistry, Stockholm.*
Count Volta, *Como.*
M. J. C. L. Simonde de Sismondi.
Baron Degerando.
Baron Krusenstern, *Member of the Academy of Sciences at St Petersburg.*
M. Oersted, *Secretary to the Royal Society of Denmark.*
M. Ampere, *Member of the Institute of France.*
M. Shumacher, *Professor of Astronomy at Copenhagen.*
M. Mohs, *Professor of Mineralogy at Freyberg.*
David Hosack, M. D. F. R. S. *New York.*
Nathaniel Bowditch, Esq. *Salem, Massachussets.*
Baron Larrey, *Member of the Institute of France.*
Sir Henry Bernstein, *Professor of Oriental Literature in the University of Berlin.*
M. De Candolle, *Geneva.*
Dr Olbers, *Bremen.*
M. Frederick Munter, *Bishop of Zealand.*
M. Oriani, *Milan.*
M. Dupin, *Member of the Institute of France.*
M. Brongniart, *Member of the Institute of France.*
The Chevalier Burg, *Vienna.*
M. Bessel, *Konigsberg.*
M. Thenard, *Member of the Institute of France.*
M. Haidinger, *Vienna.*
M. Mitscherlich, *Professor of Chemistry in the University of Berlin.*
M. Gustavus Rose, *Professor of Mineralogy in the University of Berlin.*
G. Moll, *Professor of Natural Philosophy in the University of Utrecht.*
M. Stromeyer, *Professor of Chemistry in the University of Göttingen.*
M. Hausmann, *Professor of Mineralogy in the University of Göttingen.*

LIST *of* DECEASED MEMBERS, *and of* MEMBERS RESIGNED, *from* 1823 *to* 1826.

1822.

Nov. 6. C. L. Berthollet, Member of the Institute of France.

1823.

Mar. 9. M. Van Swinden, Amsterdam.

28. Sir Ilay Campbell, Bart.

May 2. Right Honorable Lord Glenbervie.

July 8. Sir Henry Raeburn.

Sept. 23. Dr Matthew Baillie.

Right Honorable The Earl of Hopetoun.

Dr Farquharson.

Dec. 31. Dr Rogerson.

Rev. Dr A. Bell.

1824.

Apr. 27. The Most Noble the Marquis of Lothian.

Oct. 2. Dr Robert Groat.

Captain Robert Hay, R. N.

1825.

Apr. 19. Professor Pictet.

M. Breislak.

General Vyse.

Resignation.

April 7. 1823. Dr Borthwick Gilchrist.

LIST *of* PRESENTS *received by the* ROYAL SOCIETY OF EDINBURGH. *Continued from* Vol. IX. p. 541.

	PRESENTS.	DONORS.
1822.		
	The Door of the Bookcase of Sir Isaac Newton.	John Robison, Esq.
1823.		
Mar. 17.	The Vertebra of a Whale, found in the Blue Clay near Dingwall. See Transactions, vol. x. p. 105.	Sir George S. Mackenzie, Bart.
Apr. 7.	Account of a Voyage to Greenland, by Captain Scoresby.	Captain Scoresby.
	Account of Mr Clissold's ascent of Mont Blanc.	Mr Clissold.
	System of Practical Nosology, by Dr Hosack, of New York.	Dr Hosack.
May 19.	Meteorological Journal kept at Lerwick for 1821, by Dr Scott.	W. C. Trevelyan, Esq.
July 1.	Acts of the Parliament of Scotland, vol. x.	The Commissioners of the Public Records.
Nov. 3.	Transactions of the Horticultural Society of London, vol. v. part ii.	Horticultural Society.
	Asiatic Researches, vol. x.	Asiatic Society.
	Mémoires de l'Academie Imperiale de Petersburg, vol. viii.	Imperial Academy of Sciences of St Petersburg.
1824.		
Jan. 12.	Meteorological Register kept in Van Dieman's Land.	His Excellency Sir Thomas Brisbane.
	Transactions of the Horticultural Society, vol. v. part 3.	Horticultural Society.

1824.	PRESENTS.	DONORS.
Jan. 12.	Antiquarian Annals of Copenhagen, 6 vols.	Mr Frederick Munter, Bishop of Zealand.
	Transactions of the Royal Society of Stockholm for 1822. 3 vols.	From the Royal Society of Stockholm.
Feb. 16.	Transactions of the Batavian Society.	From the Society.
	Flora Batava, No. 64.	His Majesty the King of the Netherlands.
Apr. 19.	The Philosophy of Apparitions, by Dr Hibbert.	Dr Hibbert.
	Flora Batava, No. 65.	His Majesty the King of the Netherlands.
May 3.	Observations Medicales faits pendant les Campagnes de Russie 1812, par M. Le Chevalier Kirchoff.	M. Le Chevalier Kirchoff.
	Memoirs of the Wernerian Society, vol. iv. part ii.	The Wernerian Society.
June 7.	Transactions of the Cambridge Philosophical Society, vol. ii. part i.	Cambridge Philosophical Society.
	Mémoires de la Societé Naturelle de Geneve, vol. i. part i. and ii ; and vol. ii. part i.	Society of Natural History of Geneva.
Nov. 15.	Transactions of the Horticultural Society of London, vol. v. part iv.	Horticultural Society.
	Report of the Garden Committee.	Do.
	List of the Members of the Horticultural Society.	Do.
	Transactions of the Royal Society of Cornwall, vols. i. & ii.	Royal Geological Society of Cornwall.
	Catalogue of the Library of the American Philosophical Society.	American Philosophical Society.
Dec. 6.	History of the Introduction of Christianity into Norway, by M. Frederick Munter, Bishop of Zealand.	M. Frederick Munter, Bishop of Zealand.
	Narratio de Lucio, Primo Episcopo Romano, by M. Frederick Munter.	Do.
	Account of the Bell-Rock Lighthouse, by Robert Stevenson, Esq.	The Commissioners for the Northern Lighthouses.

1824.	PRESENTS.	DONORS.
Dec. 6.	Acts of the Scottish Parliament, vol. xi.	The Commissioners of Public Records.
	Medical Essays, by Dr Hosack.	Dr Hosack.
	The following Works were presented by Dr Zechinelli:	
	1. Alcune Riflessione Sanitario-Politiche sulla Pelagra.	Dr Zechinelli.
	2. Narrazione dell' origine del Tiso contagioso che ha regnata nella Citta di Padua.	
	3. Progetto per un Regolamento delle condotte Mediche.	
	4. Memoria supra il Suspirium de Seneca.	
	5. Considerazione sulla Angina dell' Petto, e sulle Morte repentine.	
	6. Ricerche sul Indole, e sulla cura della Febbro Gialla.	
Dec. 20.	Astronomical Observations of Professor Bessel of Konigsberg.	Professor Bessel.
1825.		
Jan. 17.	A Cast taken in Plaster of one of the Stones on Corstorphine Hill, supposed to bear traces of having been acted on by the attrition of stones carried along by a great current of water.	Sir James Hall, Bart.
Feb. 7.	Memoirs of the Astronomical Society of London, vol. i. part. ii.	Astronomical Society.
	Mémoire sur l'Affinité des Corps pour la Calorique, par M. Le Chevalier Avogadro.	M. Le Chevalier Avogadro.
	Dissertatio Inauguralis, by C. F. Bellingeri.	Dr Bellingeri.
	Memorie delle Reale Accademia delle Scienze di Torino, Tom. 28.	Royal Academy of Sciences of Turin.
	American Journal of Science, by Professor Silliman, 8 vols.	Professor Silliman.
	The Septenary System of generating Curves by continued motion, with various specimens of curves thus generated, by J. Jopling, Esq. Architect.	J. Jopling, Esq.

1825.	PRESENTS.	DONORS.
	Transactions of the Society of Arts, vol. xlii.	The Society of Arts.
Mar. 7.	Discourse read at the Annual Election Meeting of the Caledonian Horticultural Society, by Dr Duncan *senior*.	Dr Duncan *sen.*
	Inaugural Discourse read before the New York Horticultural Society, by Dr Hosack.	Dr Hosack.
	Description of a Breathing Pump, invented by M. Van Houten, Amsterdam.	M. Van Houten.
	Elementary Treatise of Mineralogy and Geology, by Professor Cleaveland, 2 vols.	Professor P. Cleaveland.
	Meteorological Register kept in the Isle of Man, by Robert Stewart, Esq. Receiver-General.	Robert Stewart, Esq.
	The Snout of a Saw-Fish.	The Marchioness of Huntly.
April 4.	Memoirs of the Royal Academy of Sciences of Berlin, 20 vols.	The Royal Academy of Sciences of Berlin.
	Flora Batava, No. 67.	His Majesty the King of the Netherlands.
	Transactions of the Horticultural Society of London, vol. v. part 5.	Horticultural Society.
	Traité General des Pese-Liqueurs, par M. N. Benoit.	M. Benoit.
	Mémoires de l'Academie Royale des Sciences de l'Institut de France, 1819 and 1820.	The Royal Academy of Sciences of Paris.
	American Journal of Science, vol. ix. part. i. and ii.; conducted by Professor Silliman.	Professor Silliman.
	Transactions of the Linnean Society, vol. xiv.	Linnean Society.
Dec. 5.	The Edinburgh NewDispensatory, by Dr Andrew Duncan *jun.*	Dr A. Duncan *jun.*
	Astronomical Observations, by Professor Bessel.	Professor Bessel.
	Observations on the apparent distance and position of 380, Double and Triple Stars, by J. F. W. Herschel, Esq. and James South, Esq.	The Authors.
Dec. 19.	Astronomische Nachrichten, No. 64–72.	Professor Shumacher.

1825.	PRESENTS.	DONORS.
	Observations made at the Heligoland Observatory.	Professor Shumacher.
1826.		
Feb. 20.	Enumeratio Euphorbiarum quæ in Germania et Pannonia gignuntur.	Dr Roeper.
	Disposition Methodique des Especes des Mousses, par G. A. Walker Arnott.	Mr Walker Arnott.
Mar. 10.	Transactions of the Horticultural Society of London, vol. vi. p. i. and ii.	Horticultural Society.
	Cours de Medecine Pratique, par M. Leroux.	M. Leroux.
	Description d'un Appareil Electro-Dynamique, par M. Ampere.	M. Ampere.
	Précis de la Theorie des Phenomenes Electro-dynamique, par M. Ampere.	M. Ampere.
April 3.	Memoirs of the Astronomical Society of London, vol. ii. part i.	Astronomical Society.
	Journal of the Academy of Natural Sciences of Philadelphia, 4½ vols.	From the Academy of Natural Sciences of Philadelphia.
	Transactions of the American Philosophical Society, vol. ii. New Series.	American Philosophical Society.
	Transactions of the Society for the Encouragement of Arts, vol. xliii.	Society of Arts, London.
May 10.	American Journal of Science, Vol. x. No. i. by Professor Silliman.	Professor Silliman.
	Physicalische Beschreibung der Canarischen Inseln, par Leopold Von Buch. Berlin, 1825.	Baron Von Buch.
	Atlas for Do.	Do.
	Memoirs of the Wernerian Society, vol. v. part ii.	Wernerian Society.
	Memorie delle Reale Accademia delle Scienze di Torino, Tom. 29.	Royal Academy of Sciences of Turin.
	Mémoire sur la Densité des Corps solides et liquides comparées avec la Grosseur de leurs Molecules, et avec leurs nombres affinitaire, par le Chevalier Avogadro.	The Chevalier Avogadro.